# B I O N S

## ON THE GENESIS OF VEGETATIVE LIFE

WILHELM REICH — EXPERIMENTAL BION PRODUCTION

- The Experiment (Provisional Report)
- The Dialectical Materialist Interpretation

ROGER DU TEIL — THREE SERIES OF EXPERIMENTS ON THE BASIS OF THE "TENSION-CHARGE" PRINCIPLE

ARTHUR HAHN — THE HISTORY OF CONCEPTIONS OF THE ORIGIN OF ORGANIC LIFE SINCE THE 17th CENTURY

# EXPERIMENTAL BION PRODUCTION

by

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WILHELM REICH EXPERIMENTAL BION PRODUCTION

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N.B. Re: Die Bronn Translation

ABBREVIATIONS.

| | |
|---|---|
| Abh. theoret. Biol. | Abhandlungen zur theoretische Biologie. Ed. by J. Schaxel. |
| Am. J. Sci. A. | American Journal of Science and Arts. |
| Arch. Entwmech. | Archiv für Entwicklungsmechanik. Ed. by W. Roux. |
| Beitr. Biol. Pfl. | Beiträge zur Biologie der Pflanzen. Ed. by F. Cohn. |
| Ber. Deuts. Bot. Ges. | Berichten der Deutschen Botanischen Gesellschaft. |
| Ber. ges. Physiol. exper. Pharm. | Berichte über die gesamte Physiologie und experimentelle Pharmakologie. |
| Ber. wiss. Biol. | Berichte über die wissenschaftlich Biologie. |
| Biol. Zbl. | Biologisches Zentralblatt. |
| Bot. Z. | Botanische Zeitschrift. |
| Berl. Tierärztl. Wschr. | Berliner Tierärztlichen Wochenschrift. |
| Zbl. Bakt. | Centralblatt für Bakteriologie. |
| Zbl. Min. | Centralblatt für Mineralogie, Geologie und Palaeontologie. |

| | |
|---|---|
| Encyc.Brit. | Encyclopaedia Britannica. |
| Erkenn. | Erkenntnis. Ed. by Carnap and Reichenbach. |
| Ergebn.Anat.Entwgesch. | Ergebnisse der Anatomie und Entwicklungsgeschichte. |
| Ergebn.Biol. | Ergebnisse der Biologie. |
| Ergebn.Physiol. | Ergebnisse der Physiologie. |
| Gesamm.Abh.Entwmech.Org. | Gesammelte Abhandlungen über Entwicklungsmechanik der Organismen. |
| Handb.biol.Arbmeth. | Handbuch der biologischen Arbeitsmethoden. Ed. by E.Abderhalden. Berlin. 1921. |
| Handb.normal.u.pathol. Physiol. | Handbuch der normalen und pathologischen Physiologie. |
| Handb.Pflanzenanat. | Handbuch der Pflanzenanatomie. |
| Handb.pathog.Mikroorg. | Handbuch der pathogenen Mikroorganismen. Ed. by W.Kolle,R.Kraus and P.Uhlenhuth. 3rd ed.½,1.Jena and Berlin/Vienna. 1929. |
| Handb.Naturw. | Handwoerterbuch der Naturwissenschaften. 2nd ed.Jena.1925. |
| Klassik exak.Wiss. | Klassiker der exakten Wissenschaften. Ed.by Ostwald. |
| Klin.Wschr. | Klinische Wochschrift. |
| Maandbl.Natuurwet. | Maandblad voor Natuurwetenschappen. |
| Med.Klinik. | Medizinische Klinik. |

| | |
|---|---|
| Nova.Acta.Leop.Carol. | Nova Acta der Leop.-Carol. Akademie. Halle. |
| N.Jahrb.Min. | Neues Jahrbuch für Mineralogie. |
| Naturwissenschaften. | Naturwissenschaften. |
| Naturw.u.Landw. | Naturwissenschaft und Landwirtschaft. |
| Pfluegers.Arch. | Pfluegers Archiv für die gesamte Physiologie. |
| Philos.Trans. | Philosophical Transactions. London. |
| Proc.Bgham.Nat.Hist.Soc. | Proceedings of the Birmingham Natural History and Philosophical Society. |
| Proc.Camb.Phil.Soc. | Proceedings of the Cambridge Philosophical Society. |
| Proc.Roy.Soc. | Proceedings of the Royal Society. London. |
| Schr.wiss.Welt. | Schriften zur wissenschaftlich Weltauffassung. |
| Vortr.A.Entwmech.Org. | Vorträge und Aufsaetze über die Entwicklungsmechanik der Organismen. |
| Vjschr.öff.Gesundhpfl. | Vierteljahrschrift für öff. Gesundheitspflege. |
| Z.allg.Physiol. | Zeitschrift für allgemeine Physiologie. |
| Z.Hyg. | Zeitschrift für Hygiene. |
| Z.Kristal. | Zeitschrift für Kristallographie. |

| | |
|---|---|
| Z.Phys. | Zeitschrift für Physik. |
| Z.pol.Psychol.u.Sex. | Zeitschrift für politische Psychologie und Sexualökonomie. |
| Zbl.Physiol. | Zentralblatt für Physiologie. |

Chapter II.

BIONS AS A PRELIMINARY STAGE OF LIFE.

The vegetative currents which I encountered in character-analytical work and in the electrical experiments relating to sexuality, induced me to pursue them further in Protozoa under the microscope. The Institute of Botany in Oslo placed a protozoal preparation at my disposal. It was my intention to make preparations of this kind myself and to this end I made enquiries as to the technique; I did not want to rely on the knowledge of biological practice which I had acquired two decades ago and then forgotten. Although I was acquainted with Leeuwenhoekian infusions I was very surprised to hear that it consisted of nothing further than an infusion of water on hay. I also learnt that Amoebae are frequently found on leaves which have lain for a long time in pools of standing water. I blushed for the gaps in my biological knowledge when I received the astonished answer to my naive question as to how the animals got into such an infusion, that there were "germs of life" everywhere from which the Protozoa developed. I wanted to study special Amoeba cultures in order to become acquainted with the plasma currents described by Hartmann and Rhumbler, which were so important for my theory of vegetative function. But in the infusion I had taken over I found only very few Amoebae. It turned out that it was not so easy to produce new Amoeba cultures. The Institute assistant advised me to make infusions of hay myself and to examine them "after about ten to fourteen days". Then I should certainly find Amoebae.

My knowledge of protozoology as a specialist subject was shamefully small. But with confidence in my thorough knowledge of theoretical biology and supported by the orgasm theory and the experimental experiences of the last few years I felt justified in launching out into these new waters. To begin with I intentionally avoided reading afresh the specialist biological literature in order to be able to proceed with my observations free from preconceived ideas. I left the collection of literature to an assistant.

1. THE FORMATION OF VESICLES IN INFUSED GRASS.

In the preparation Paramecia, various kinds of Amoebae and worm-like serpentine structures could be clearly observed. The vegetative streaming of the plasma struck one forcibly. The slow serpentine movements and streaming of the

plasma could be easily recognised as the tension-charge process I was seeking. I made the experiment of sending a current of 0.5 mA through the preparation and saw, as described by Hartmann and Hhumbler, that the streaming of the plasma increases in speed when weak currents are passed. The Amoebae moved more rapidly and the granular movements within the cell, observed at a magnification of 1500x, became extremely lively. The Paramecia, on the other hand, appeared to be in a state of great disorder; their rapid and continuous movement ceased, they began to turn round in circles; it looked as though every application of current had a "traumatic" shock effect. After a longer current flow, about 3 minutes, the whole field lay motionless except for the Amoebae. I increased the current to about 1.5 mA; the Amoebae also rounded up and ceased to move.

These observations seemed to me important, but I was not yet able to establish the connection between the change in the streaming of the plasma due to the passage of current and my hypothesis. There was, however, another point of attack for the testing of the tension-charge formula.

Living organic tissue visibly absorbed fluid, so that the membrane tensed mechanically in the process of swelling. The so-called "turgor" of tissue - filling up of blood vessels, hyperaemia - consists in principle of nothing more than a greater absorption of fluid (blood, lymph, water). Hence I concentrated my attention on the changes which took place at the edges of the plant fibres.

In the course of some two days the shimmering green fibres became colourless. There were many green chloroplasts free in the fluid. But remarkable changes took place where the plant fibres lost their colour and disintegrated. The previously striated and unicellular structure gave way to a vesicular one. Simultaneously the edges projected at isolated points, mostly in semi-spherical form, but also in zig-zag strips.

The fibre had obviously absorbed water and was swollen, the chloroplasts had detached themselves, and vesicular disintegration had set in in the cellular structure of the tissue. Heaps of vesicles, marginless and motionless, lay everywhere in the previously clear fluid. They had precisely the same structure as the vesicularly-disintegrated plant edges. The vesicular structure varied at different points - here irregular, without a clear margin; there entirely regular and surrounded by a margin which shone out clearly when the microscope was brought into focus. Where there were

well-defined margins the vesicular mass was as if "formed"; the more complete the margin, the more fully-formed was the structure. The vesicles gradually broke away from the fibres and floated freely in the fluid. The plant fibres then looked like leafless twigs (Illus. 12, 13 & 14).

I now followed the changes in both the vesicular-organisation and the margin-formation in one structure for four hours. On the edge of a piece of plant tissue there was an irregularly-structured, marginless, vesicular formation. It slowly swelled, became larger and broke away from the plan. It became surrounded by a distinct refractile margin. The vesicular structure was now more regular, more homogeneous, the vesicles stood out more clearly against the light. The formation was hardly distinguishable in form from an Amoeba which happened to move past. It assumed an ovoid shape and became fuller the more complete and well-defined the margin grew. The internal vesicles lay motionless, but the whole had assumed "form". The formation disappeared at the attempt to add water to the preparation.

An Amoeba which moved slowly into the margin area dried and rounded up. It could now no longer be distinguished from the many heaps of vesicles which lay about.

Another observation pointed in the same direction. Formations can be observed which exhibit the vesicular structure described, together with a well-defined margin, and are attached to the plant fibre by a stalk. Occasionally one sees a formation of this kind making vigorous movements to tear itself free. It moves rapidly away from the main body, but is dragged back again by the threadlike stalk. The important thing is that it differs from the inanimate formations only in its motility, while form and content appear the same. The many vesicularly-structured forms which move quickly about the field seem also to be ofthe same kind.

To summarise the observations:

1. Infused plant fibres disintegrate vesicularly.
2. Dried-up Amoebae have a vesicular structure similar to heaps of vesicles.
3. Vesicular, margined formations actively tear themselves away from the disintegrating plant tissue.
4. In the course of formation the well-defined margin gives the heap a definite form and fulness.
5. Many swiftly-moving cells show a sharp margin and evenly-vesicular structure.

The astonishing assumption thrust itself forward that the vesicular (honey-combed) living plasma of the Amoeba must

have a very close connection with the heaps of vesicles formed in the disintegration of plant tissue. Is it possible that an Amoeba or other vesicularly-structured Protista are nothing else than heaps of vesicles encompassed and given form by a margin?

The further observations and experiments speak best for themselves.

2. ANIMAL TRANSFORMATION OF GRASS AND MOSS TISSUES.

Using a certain kind of grass in an infusion, the types of Protista I have described were formed in a series of preliminary stages from the disintegrating plant fibres. Immediately after it was immersed the leaf of grass showed a well-defined dentate margin (Illus. 15). In the course of some 24-48 hours this margin disintegrated vesicularly as described (Illus. 16). On about the third day I was able to observe round, spherical to ovoid vesicular formations, containing granules or vesicles, at irregular intervals on the margin of the grass (Illus. 17, 18 & 19). The vesicles within the large, margined vesicular formation cannot be distinguished from the vesicular indentations of the plant margin. The large round formations are attached to the plant margin by a stalk. Some of them do not change this shape, but others exhibit a noteworthy behaviour. The spherical masses which were extruded from the plant margin slowly extended and an opening surrounded by the finest fibres appeared anteriorly. The formation maintained this extended position for some 1-3 seconds till it contracted and suddenly resumed a spherical shape. These formations have the appearance of round apples or long olives swinging on a bough. They can also be seen either at the same time or somewhat later, some swimming free in the fluid, some still attached to separate pieces of plant tissue by a stalk. Their movements are the same as those described. The extension and subsequent contraction to a spherical shape can be observed for hours. Occasionally such a formation can be seen to move away from the plant in the process of extension and in so doing to stretch the wound-up stalk. In contracting many return rapidly to the plant-matrix. Is this to be regarded as already "animal" or is it still "vegetable"? Such a question is incorrect. It is a transitional form, for the same preparation two days later showed the same animal free in the fluid not attached to the plant tissue; but to some extent they already had a different character. They could be seen in the most various stages of development. There were still vesicular spherical formations attached to a plant fibr

plant via a series of developmental phases. To classify or assign it to known classes at this stage would offer little advantage. A film was made of the preparation observed (preparation 8). The contractile formations were called "org animalcules" (after "orgastic" contraction).

3. THE VESICULAR NATURE OF AMOEBAE IS CONFIRMED.

(G) 1936

Moving Amoebae were first assumed to be collections of vesicles about two years ago. The direction of earth-bion investigation at first led away from the Amoeba problem and confirmation of this assumption was not possible. Microscopi observations of a strict kind were lacking. Hope now rested on a low-speed apparatus which would reveal the process photographically. But for a year technical difficulties prevented solution of the problem. Low-speed photography demanded a completely static preparation free from fluid movements. It soon proved that grass preparations immersed in paraffin die through lack of oxygen. But a film of the encompassing of isolated vesicles by a margin and the development of Amoebae therefrom might provide one of the principle pieces of proof. The technical problem was solved as follows. Two or three pieces of teased moss or grass were placed on a cavity-slide and the fluid of the preparation added. A cover-glass was fitted with wax legs at all four corners and laid over it without air bubbles forming. The cover-glass was now affixed to the two long sides of the slide with paraffin. Care has to be taken that about a quarter of the cavity on the slide is not covered by the cov glass. On the two sides of the cover-glass not covered with paraffin, water is now piled up so as to form reservoirs fro which the evaporating fluid is continually renewed. Both reservoirs have to be replenished at approximately 2-hourly intervals. By these means, filming over a period of days becomes possible, though only when great care is taken that the water reservoirs are replenished so that the preparation does not dry up. It could now be demonstrated that under certain, not yet clearly established, conditions, dried moss in water produced Amoebae, amongst other Protozoa. It is now possible to follow their growth and the way in which a well-defined spherical margin forms round groups of vesicles. Onc a certain size is reached, one part of the margin swells out, while the whole remains motionless. In the centre of the mas appears a small sphere. The large marginal vesicles each contain a small, almost dot-like, vesicle in their centre, and their peripheries become indistinct and merge into each other forming an ectoplasmic mass. The vesicles in the interior of the large round formation are smaller and form coarse,

vesicular endoplasm.

After some hours the formation breaks free from the fibre and moves away as an Amoeba. Only in the final phase of the process does the marginal swelling begin to move. The breaking-free takes place slowly. Once the formation has broken away from the fibre the plasma can also be seen swelling out on several sides of the organism. The formation of Amoebae proceeds in this way uninteruptedly: Sometimes two or three together in a short space of time, sometimes one after the other at intervals. The free Amoebae separate. They can often be seen lying side by side in colonies. Like this they are scarcely distinguishable from the ends of swollen moss fibres. Proof of the genesis of Amoebae from infused moss fibres was provided by direct observation (cf. the micro-photos from the low-speed film No. 8: 26-31).

## 4. MOBILE VESICULAR EARTH CRYSTALS AND EARTH BIONS.

My observations and the conclusions to which they led stood in direct contradiction to the "germ theory". I intentionally avoided orienting myself afresh in the ruling concepts of the "origin of life from life germs". Work that was entirely free from preconceptions was even more indicated after these first observations than before. I put down several non-sterile cultures for comparison: a tulip petal, a rose petal, grass and plain earth. After 3 days the tulip and rose petals showed no Protozoa; the grass infusion was full of mobile bacilli, vesicles and various Protista; the earth infusion (preparation 1a) afforded great surprises.

Microscopic examination immediately after infusion showed sharply-margined, structureless crystals which lay completely motionless (Illus. 32). Isolated vesicular formations could be seen in jerky motion between the crystals here and there shimmering, green, longish, anuclear bacilli moved slowly about. On the third day the same preparation, which had remained open, afforded a different picture. It was full of mobile formations which were rectangular or rhomboidal but which nevertheless moved exactly like bacilli and vesicles. They were turgid and vesicular in form and often attached themselves to the upper surfaces of bacilli or larger formations. A large number of the crystals had changed their form; streaks had appeared within them which had here and there disintegrated vesicularly. The vesicular enclosures in the structured crystals were hardly distinguishable from the vesicular formations floating free or moving jerkily in the fluid.

On the 7th day the vesicular disintegration and the structuring had reached a very advanced stage. Already at a magnification of 700x sharply-margined processes could be clearly observed on the edges of the rectangular, irregular, brown-coloured particles of earth, which looked like vesicular tubes and alternately extended and contracted; bending movements also took place. At 1625x I discerned a brown particle whose margin showed vesicular processes in places. It was connected with another particle by a vesicular and striated mass. At the point of connection the mass bent and stretched itself. I believed at first that I was mistaken, but continued close observation left no doubt: the particle of earth moved as though it had joints and expanded and contracted.

After a further 7 days the process of vesicular disintegration, striated structuring and vesicular loosening-up of the crystal margins had advanced considerably. The vesicular processes on the margins of the crystal showed 3 kinds of movement: turning on its axis, extension and contraction, and bending.

Let us call the new formations on the crystals "plasmoids" (Illus. 36 & 37).

I passed a current of 1 mA and very slowly increased it to 2 mA. The movements distinctly increased. The jerky movement of the vesicles became quicker, stretching and bending became more pronounced. On altering the direction of the current the direction of the movements of the individuals promptly reversed. Vesicles moved towards the kathode, they were thus positively-charged particles. On disconnecting the current the movement stopped and started again on connecting the current. The tube-like processes showed jerky freeing movements on long passage of the current. These movements lasted for a short while after stopping the current. They often doubled their length.

Repeated testing of the electrical reaction always gave the same result. The passing of an electric current had therefore a still-indefinable effect upon spontaneous movement, particularly extension.

We have so far seen two probably basically different phenomena:

1. the vesicular disintegration of organic tissues.
2. the vesicular structuring of inorganic earth crystals followed by the formation of mobile tubes and other mobile forms.

From now on I replaced the water in the preparation by 0.1 normal potassium chloride solution in order to test the swelling effect of potassium. I saw that earth treated with potassium chloride swelled up quicker and the described appearances were more distinct. Likewise the treatment of completely or partially dried grasses; as with the KCl-earth preparations, the vesicular disintegration could always be produced.

I now added highly diluted red gelatine to the disintegrated grass and earth preparations. After a few hours, and even more so after one or two days, I could see amoeboid forms which had not been there previously and which were still not present in those preparations without gelatine. These forms, which I called "pseudo-amoebae", crept about in various directions with jerking movements. There were also heaps of vesicles on whose margins there stood up small isolated rods like moving nails. The movements were jerky, not of an organic flowing nature such as the streaming of the protoplasm of Amoebae. Besides the locomotion, one could also see at 2000x internal movements such as bending and stretching. Thus the gelatine had collected the heaps of vesicles together into a series of vesicles, exactly in accordance with my assumption (that I would be able to coalesce the earth vesicles by the addition of some sort of artificial material). These heaps of vesicles now distinctly moved. What was the cause of these movements? Flowing of the plasma as in Amoeba could not be seen. Illus. 34 shows a completely vesicularly-structured crystal which was connected by a stalk to a second which was only structured on the left-hand margin. This crystal, at the time of the exposure, was moving quite slowly in the direction of the arrow, drawing after it the not-yet-structured crystal. Now it would seem that the described pseudo-amoebae were not the result of a synthesis but had arisen from germs in an unsterile and open preparation. To test this I now proceeded to boil the preparations for 1/4 to 1/2 an hour in closed vessels.

This experiment produced a completely unexpected result. These preparations showed a much richer and more vigorous life immediately after boiling than the unboiled preparations after days of maceration. In the unboiled preparations the vesicles were strewn about isolated, and the unstructured crystals were in the majority. The vesicular structuring of the crystals proceeded slowly over the course of weeks. On the contrary, the boiled preparations immediately showed an innumerable number of vesicles, closely arranged and vigorously and constantly moving about, and also irregular vesicles.

The fluid of the unboiled preparations ordinarily remained clear and had a deposit. That of the boiled preparations became opaque, like a colloid. Also, electrical checking of the boiled preparations showed that the vesicles were positively charged, and on applying a current of 1/2 to 1 mA they migrated to the kathode. On later addition of sterile diluted gelatine the same result was obtained as with unboiled preparations. The same result was obtained by boiling a mixture of earth, KCl and gelatine.

But the presence of mobile forms in a boiled and sealed preparation immediately after boiling presented me with an important question. How could it be possible that the boiled preparation could show much more life than the unboiled, unsterile and open ones? That would seem to contradict all the laws of sterilisation. Six days after boiling, a sealed preparation showed that most of the forms exhibited the movements at 1500x. There were also plasmoid processes and forms, refractile points with 3 or 4 thread or rod-like processes.

Most of the crystals were completely vesicularly-organised. Periodic controls with fresh unboiled earth-water preparations always showed the same lack of vesicular organisation and also much smaller numbers of vesicles.

By using more powerful lenses with water-immersion, magnifying 2300-3000x, I could observe distinct pulsations in certain spots of the contractile forms. Solid crystalline forms with structured margins, which seemed to be bound together as by gelatinous material, sought "to free themselves". In many clumps I saw very mobile, green, refractile forms within the gelatinous mass, and these differed in no way from bacilli swimming freely in the fluid. I was forced to assume that the lumps of earth had swollen up and had in part been transformed into vesicles and bacilli. It depends on yet-unknown factors whether these swollen units remain in the crystal or free themselves and swim around in the fluid. These preparations also showed, on passing a current of 1/2 m. the already familiar phenomena of the putting out of vesicular processes, intensification of the jerking movements against one another, the kathode migration, etc.

Thus I became more than ever sure that the more vesicular is the structure of such a crystal, the more mobile it becomes. I had to assume that the vesicular particles are swollen substance-units with a high electrical charge, which are held together by gelatinous material and move

inside the amoeboid form as they also do as anuclear single vesicles in the fluid.

Repeated controls on boiled earth always gave the same results: mobile vesicles, nuclear and anuclear forms and pseudo-amoebae. In order to check whether these phenomena were due to incorrectness of technique I made control preparations with unboiled earth and tree and tulip leaves etc., which I left open. I always obtained the same result. The unsterile preparations showed much fewer vesicles than the boiled, and the formation of mobile or vesicularly-disintegrated marginal layers was much less complete.

Now there arose the question as to whether these forms really represented living substances and showed, besides motion, contractility, and other familiar properties such as cell-division. I then commenced to study the earth-KCl-gelatine-preparations more closely. By fixing one in the same field for several hours I was soon able to observe divisions of the pseudo-amoebae.

I became confident that they represented forms of life which, so to speak, lacked completeness; for example, the movements of the pseudo-amoebae were cut short, mechanical and without internal streaming. The forms must be transition stages of life. For private use I called them "bions". Could these bions develop into complete life?

BIONS

Besides division, the function of "eating" was observed and verified. I observed a pseudo-amoeboid form take in single vesicles which were swimming about free. I had to free myself from the conception which is wrongly associated with the word "eating". In a description of Protozoa we tend to err in the use of anthropomorphic expressions. Ordinarily the term "eating" connotes that a living creature swallows something from its surroundings in order to maintain its life. When I freed myself from this error of thought I was able to correlate the appearances of eating with the remainder of the observations. Now it appeared irrefutable that just as the body of a metazoon is constructed of single cells, so the cells which I had before me built themselves up from vesicles and bacilli. This contradicts the idea that cells are the ultimate units of living matter. Rather, the vesicles are the biological units while cells are more complicated structures. But these vesicles and bacilli were highly charged and turgid forms. A heap of vesicles carries out its act of eating by drawing

into itself freely swimming and similarly electrically charged vesicles which it organises into itself; exactly as with the jerking-protozoon (Vorticella-like).

Fig. 1.

Amoeba in the act of feeding

(Drawn from life)

Amoeba A did not move towards bion B, but within a certain radius the bions moved swiftly towards the Amoeba and disappeared in its interior (cf. the direction of the arrows).

I could quite well have accepted another explanation, but it seemed most important at this stage of the work to carry out further experiments and controls. The question was always before me, how is it possible that boiled preparations showed more life than unboiled? Originally I believed that the quick jerking movements of the vesicles after boiling were heating effects, but further experiments with an electric current showed that these movements were of an electric nature, for they were intensified by a current. I did not want to occupy myself yet with the problem of whether these were real bacteria or cocci or whether they were some other form. Since I had succeeded in producing pseudo-amoebae from boiled earth, KCl and gelatine it was obvious that further controls with other substances should be made. My assumptions were that there must be other substances that would remedy the incomplete character of the forms.

Later I commenced autoclaving the earth and KCl at 120°C. The result was much better than with plain coal heated to 180°C. There were fewer unstructured crystals and more exclusively mobile vesicles and heaps of vesicles. I will return to this problem in dealing with culture experiments.

The above experiments confirmed for me the biological correctness of the tension-charge formula. Vesicular swelling represented mechanical tension; the presence of electrical positivity or negativity represented the electrical charge. But much still remained obscure.

5. PROTEIN PREPARATIONS (PREPARATION 6).

By reason of the tension-charge formula, substances with the property of swelling were needed in order to complete the function of the form (pseudo-amoeba). Other experiments had shown that lecithin had the same effect as potassium and cholesterol the same effect as calcium, swelling and shrinking respectively, so I now began to add cholesterol and calcium to the preparations. As a result, a few other phenomena were seen as well as those described. The preparation was full of regular serpentine forms which slowly changed their shape.

Controls carried out with each material soon showed that these appearances could be ascribed to the lecithin. They had the following characteristics: the serpentine, often constricted, forms became longer and thicker, bent, changed their position and especially showed processes; i.e. from a spot on one tube another would branch out, which in turn also branched, etc. When I put down lecithin with KCl alone I obtained exactly the same forms, but they showed no organic movements. I thought that the "growing" and the branching that I observed could be attributed to the absorption of fluid, as in a sac. It left off after some 48 hours. Nevertheless, lecithin undoubtedly had the property of building forms. The movement of this growing and branching was fundamentally different from that of the pseudo-amoebae.

**Fig. 2**

Lecithin formations.

a) An extended tube.
b) A greenish luminous vesicle which extends and contracts on passing a current, extending to the anode.

I now added white of egg to the earth-lecithin preparation. In unboiled preparations, round cells which had a dark nucleus and exhibited rapid division were formed after a few minutes. Either the cells and nuclei divided by constriction, or else a smaller one was budded off. In some cases distinct movements of the buds to break away were seen. Thus, by the addition of protein, proper cells were formed.

Fig. 3

Cell-like formations on the addition of white of egg.

a) Nucleus-like formation in the tube.
b) Cellular formations.
c) Stages of development.
d) Stages in budding.

In the case of these protein mixtures it was also seen that the formation of mobile substances was much slower and less complete in unboiled than in boiled preparations. The latter could also be distinguished by the naked eye on account of the colloidal cloudiness of the fluid.

The colloidal particles which were formed during the boiling were thus in a state of suspension which, according to present-day knowledge, is ascribed to their electrical charge. I left the preparations to stand for several weeks and found a great increase in the number of individual forms. But after about six to eight weeks the fluid began to clarify and the particles were deposited as a thick whitish layer. Microscopically it was seen that the mobility and electrical reaction had ceased. The forms were "dead".

Summarising: lecithin and KCl alone gives no cells but only regularly-formed tubes of the most variable types. There is also no organic movement but only a growing and budding, apparently through absorption of fluid. Protein and KCl alone form no cells. But protein, KCl, cholesterol and lecithin does form cells.

I now complicated the experiments by adding gelatine. I now had together protein, earth, lecithin, cholesterol, gelatine and KCl. Immediately after boiling, plasmoid vesicular forms were seen which exhibited vigorous movements and, as opposed to the pseudo-amoebae described above, they showed a much finer structure and a much more organic vegetative movement. They were no longer jerky or mechanical but organically steady (Illus. 41). These amoeboid forms were more complete the longer the preparation was boiled. They were more fully functional than the boiled earth preparations. While the pseudo-amoebae of the earth preparations only moved from place to place, showed little or no internal movement of the granules and bending and stretchi were only shown exceptionally and then only quite weakly,

the amoeboids of the protein mixture showed streaming, internal movements, bending, stretching and locomotion. The functions of division and of "eating" were present in both cases.

The amoeboid forms of the protein mixture showed a much finer vesicular structure; and after a few days one could also see forms which had homogeneous protoplasm and a flowing, pseudopodial movement. Contraction, expansion, division and eating were present. There still remained to be answered the questions of metabolism, culture, staining, and the bacteriological nature of the forms engendered.

The production of life from non-living matter is demonstrated when the addition or omission of one or another substance changes the biological nature, structure and activity of the mobile form. The mixing of the material does not require any precise conditions for success. It is as though the forms come together according to some as yet unknown law; as though the proportions of the materials that form the mobile life were determined by an automatic princip

In later work I added to the bion-mixture meat, milk, bouillon and yolk of egg as nutriment. And later, carbon in various forms; finely powdered coal (sterilised dry), coal dust autoclaved in KCl (coal dust heated to incandescence dry, or heated in bouillon KCl), finally soot, dry sterilised or heated to incandescence. The best results were obtained by the addition of soot; there arose very small but extraordinarily mobile amoeboids of fine structure. Bion mixture 6 was now made in various ways:

6a, unsterile mixture.

6ab & ac, unsterile mixture boiled at 100°C for 30 minutes, or autoclaved at 120°C for 1/2 to 1 hou

6ac, the materials were previously sterilised.

6cb, the sterile mixture was afterwards boiled.

6cc, the sterile mixture was again autoclaved.

6ccc, the sterile mixture was autoclaved twice.

The best culture was obtained from 6cc. The first inoculation was rade after 3 or 4 days.

Can one here speak of the engendering of artificial life? In haste one would say "Yes". But after due consideration the answer must be "No". If the living constituted its own seperate metaphysical province, completel apart from the non-living, then I would be able to say that I had made "artificial life". But my experiments showed me that there are developmental stages from the lifeless and immobile to the living; that in nature the formation of life is proceeding eternally from minute to minute. In this case it is difficult to speak of "artificial life". I merely succeeded in discovering experimentally the developmental processes. Perhaps a new kind of life did also arise artificially in the process.

On 8th January 1937 I sent the following report to Professor du Teil:

"In anticipation of a detailed account I should like to give you a report on an autoclaving experiment based on the tension-charge formula. I shall give here merely the course of the experiment and its results. A film is being made at the Institute for Sex-Economic Investigation which will give a clear picture of the experiment. Specimens of the colloid preparation will also be forwarded.

"The formula, mechanical tension - electrical charge - electrical discharge - mechanical relaxation,

(Footnote: Der Urgegensatz des vegetativen Lebens, 1934. (The Primary Antithesis of Vegetative Life)

Experimentelle Ergebnisse über die elektrische Funktion von Sexualität und Angst, 1937. (Experimental findings on the Electrical Function of Sexuality and Anxiety)

Both published by the Sexpol-Verlag, Oslo.)

must be regarded as being identical with the formula of vegetative functioning in general as a basis for further experiments and on account of the many findings already made. The following experiment serves as a proof of the formula.

"At the present stage of the experiments I commence by making a mixture of 100 c.c. sterile Ringer's solution and 0.1 normal potassium chloride solution. Red gelatine solution is added to this mixture until the whole has a

pale pink colour. This is followed by the addition of a small quantity of charcoal dust and the same quantity of cholesterol crystals. The whole experiment rests on the principle of combining in a certain order those substances necessary for the formation of cells.

"A tea-spoonful of the fresh, clear white of egg is now dissolved in about 50 c.c. sterile KCl solution and the whole added to the previous mixture. The white of egg is dissolved after a little stirring. The microscopic picture reveals no formations, movement or plasmic structure. At high magnifications (1000x) we still see only the typical immobile charcoal and cholesterol crystals.

"About 1-2 c.c. of milk and some egg yolk are now added. The latter clouds the previously clear mixture.

"A second solution is now made by rubbing lecithin into KCl solution. At a magnification of 500-900x curious formations are seen to arise and grow: tubes which swell, bud and bend. No sort of structure is visible in the interio of the tubes. But occasionally formed heaps of vesicles are found. Organic movement is absent. They are purely physical swelling phenomena due to changes in the relationship between internal pressure and surface tension.

"On the addition of the lecithin solution to the first mixture, a yellowish-grey cloudiness progressively develops. The microscope shows an astonishing picture of mobile life: cell-like formations move jerkily about, bud, divide and crawl fitfully or flowingly. The organic nature of the motility can only be observed at a magnification upwards of 1500x and best at 2300x to 3000x through a binocular tube. With continuous observation at about 3000x luminous, vesicular, nuclear formations, which change their shape, can be observed. It is possible to distinguish four general groups of extremely mobile formations: round nuclear vesicles bacilli, cell-like formations with nuclei which move bodily but not internally, and finally amoeboid formations. The latter are particularly interesting as at a magnification of 3000-3500x they exhibit internal expansion-contraction movements. Their locomotion can be seen clearly at even 2000x.

"The formations which arise in this fashion are, at least according to the tests so far made, electrically negatively charged bodies; they move towards the anode. If the colloid mixture is left standing for 6-8 weeks a thick sediment forms, the fluid becoming continually clearer. If

the sediment is examined at the magnifications stated above, motility of the kind described has disappeared; the formations are "dead". A report on the control experiments carried out will be contained in the detailed account. At present investigations are being made into assimilation and sensitivity to colour. It is definitely established that the formations are culturable. If the substance remaining after all fluid has been boiled away is inoculated on a sterile colloid mixture, growths arise which have so far been cultured up to the fifth generation.

"In vivisection experiments it proved that the formation if kept sterile, are uninjurious when injected subcutaneously in mice and guinea-pigs.

"The question as to whether we are dealing with complete living formations can only be definitely answered in connection with other experiments which will be reported later, and after all tests and controls have been carried out. The experiment described has been photographically recorded.

"The artificial, lifelike formations were named "bions".

Chapter III.

THE CULTURE OF BIONS (PREPARATION 6).

Bions are preliminary stages of life, transition forms from the inorganic and immobile to the organic, mobile and culturable. In order to develop the bions, whether from earth, carbon, animal tissue, moss or from mixtures, it needs very careful observation in order to determine the conditions which bring them over the border of culturability. That they are stages and not complete life becomes clear only through the difficulties met with in attempts at culture. The materi constituents of the bions are as decisive as the ingredients of the culture medium.

N.B. Culture medium C. Ringer's solution and KCl solution were mixed, then some red gelatine was added until the fluid was a pale pink colour. Then was added carbon, cholesterol, a quart of white of egg, a few drops of milk and some yolk of egg. Finally some lecithin and a whole egg is added to the fluid. This mixture is distributed into the culture glasses and dried out in the glasses in a horizontal position for an hour at 80-100°C. There remains a drop of condensed water. The colour of the medium is yellow.

Culture medium D. The same preparation as C. In some vessels growths occur without inoculation (auto-inoculation).

Culture medium E. Some Ringer's solution and KCl were mixed. To this were added a few drops of red gelatine dissolved in KCl solution and some powdered coal boiled in KCl solution. Also a dessertspoonful of milk and a whole egg plus lecithin rubbed in KCl. This was distributed in the culture vessels and dried out for 2 hours at 80-100°C. This is done for two days in succession. Auto-inoculation does not occur. This was controlled by leaving the vessels for 5 days at 37°C without a growth developing.

Medium F. Ordinary bouillon and agar medium.

A.d. 1936
1 Jan 1937

In my laboratory since Autumn 1936 we made on an averag two or three fresh bion preparations (6) each week. On the 1st January 1937 a fresh bion mixture which had been boiled for an hour was inoculated on a medium. This medium A consisted of the dried residue from the complete evaporation of the bion substance. On the 3rd January, i.e. 48 hours later, the medium showed a grey-white moisture which constantly increased. The medium was in a state of

dissolution. The medium, as well as the bions, was made sterile by several hours' boiling. This medium A had stood for weeks in a thermostat without showing any signs of dissolution. The first culture generation seemed to have been obtained. Microscopically there were four types of bions: round vesicular cocci, shorter and longer rods, round nucleate cells, and contractile and expansible creeping amoeboid types. The culture forms seemed to be more mobile and powerful than the original boiled culture. It is seen from the protocol scheme that this first stock succeeded in reproducing to the 9th generation under variations in the culture medium. The reproduction suffered only one interruption, on the 9th February, because the culture medium had dried up. The further inoculation of this third generation followed on the 14th February on medium B. This I obtained by incomplete evaporation of the residue. Apart from this one interruption the reproduction proceeded undisturbed. Regularly within 24-48 hours the previously dry medium became moist and grey-white. In the fluid there were forms of mobil character. The un-inoculated control media up to medium D remained unchanged, i.e. they showed no dissolution. If the medium was not sufficiently dried out, so that some fluid remained, it sometimes happened that auto-inoculation occurred as mentioned above. Apparently, in spite of the long boiling, some bions remained to cause dissolution of the medium. Nevertheless, the self-inoculation could not be considered a negative sign; for if the bions are not destroyed by boiling, and the slightest trace of moisture in the mixture calls forth mobile life, then it is conceivable that the medium itself lives. Rather than attemp to exclude self-inoculation, can we find a medium that has not the property of self-inoculation?

Before I hit on the idea of evaporating the medium mixture and using the substance as medium, weeks of research on reproduction in Ringer's solution gave no clear result. On the contrary, the following experiment proved a complete surprise. From the 7th generation of the 1st stock of the sterile 6b preparation I boiled a culture for 15 minutes in a steriliser. The boiled culture was now (8th February) inoculated on a new variation, E, of the old bion medium. At the same time the 7th generation was inoculated on bouillo and agar, both boiled and unboiled. Already on the 9th February it was seen that both the boiled and the unboiled cultures had grown into thick heaps on the agar and protein mixture. The agar medium was not dissolved. Macroscopically there was no difference between the boiled and the unboiled preparations. Although this result may seem surprising at

first sight, it is really obvious; for if the bions should finally develop into living forms which are not destroyed by boiling, then it is only logical that the cultures should be able to give further cultures after boiling. This experiment has already been confirmed.

On the 9th January a second, fresh, 2-day old, sterile bion stock was inoculated on medium E. On the 11th January the medium in one glass had been dissolved by the stock. The control medium had not been dissolved, neither had a second inoculated medium.

At this stage in my work it repeatedly happened that a bion stock would not grow in the 1st generation on the media so far used. Their stock was now, after failures with A and B, successfully inoculated on E. On the 22nd January there was distinct dissolution in two glasses. From this 2nd culture generation of the 2nd stock I inoculated medium E and obtained a new growth the next day. On the 27th January I inoculated a boiled 2nd culture generation on the same medium E and obtained a similar growth on the 4th February. The boiling test succeeded with stock 2 just as with stock 1. We continued the unboiled culture growths, as designated in the table, up to the 9th February, always with positive results. On the 8th February the boiled culture generation 2b II was re-inoculated on medium B. Already 24 hours later there was a growth in the form of grey-white, milky, watery dissolution of the medium (2b III).

The same thing happened with the 3rd stock of bions which was inoculated on culture medium D on the 21st January.

I now want to describe some of the fundamental phenomena to be taken into consideration in the care of successful and unsuccessful cultures. After 48 hours, often much later, in the case of successful cultures the surface of the medium becomes clear, bright and shiny, like a film of water. Careful comparison of the control medium with the help of a lens fails to show mounds and the surface sheen of the film. The control media remain smooth on the upper surface when when they do not undergo dissolution. In the case of inoculated media, on the contrary, there appear mounds which are mostly round or sometimes elongated in appearance and grey-white in colour. Medium D shows drops of clear condensed water but this water usually dries out of the control media in a thermostat, while the exuded fluid of the inoculated media becomes opaque and eats deeply into the medium. Occasionally a reddish growth appears, the reasons for which have not been

discovered. It is very important that the later generations give new growths much more easily than fresh mixtures. The later generations are also much more mobile, more complicated in structure, and more perfected. The four fundamental forms (round vesicles, long mobile rods, nucleated cells, and amoeboid forms) are again found everywhere, but by long and careful observation up to a magnification of 4000x it is also seen that the types are not distinct, each type can be subdivided into other types.

After many successful but not finally substantiated attempts at culture on egg-yolk medium we succeeded in culturing bions on bouillon and agar in the following way. Bion mixture 6 was heated for 1/2 hour in test tubes in a dry steriliser at 160°C. The heated mixture was kept sterile for from 2-4 days and inoculated in the following way on bouillon and agar medium. A drop of the mixture was flamed and transferred to the test tube, the mouth of which was also flamed. The first bouillon inoculations with three different bion mixtures gave without exception a strong cloudiness within 24 hours.

Macroscopically one could distinguish in the bouillon cultures:

a) the cloudy opacity.

b) the flakes.

c) a fine thready and dusty cloud which rose up and increased the opacity on shaking the test tube.

The growths on the agar media were macroscopically less unequivocal. After 24 hours there was sometimes a very thick yellowish growth, but usually there was merely a very thin growth, indistinctive in colour, which became somewhat thicker after a few days.

By microscopic examination all the bouillon cultures showed various types of rods, which were mostly very mobile, and cocci, but very few amoeboid forms. The thicker agar growths proved microscopically to consist predominantly of very mobile cocci. The weak agar growth showed much less movement, but if one transferred the second generation of the weak agar growth into bouillon, then the bouillon cultures showed the same forms and the same macroscopic appearance as the same bouillon inoculation. Thus the medium influences the kind of culture. If one inoculates from bouillon on to agar, a growth regularly forms within 24 hours. Occasionally

in re-inoculation from bouillon to agar, the cocci again predominate, while by re-inoculation in bouillon the rods and amoeboid forms again appear.

In order to be completely certain about the cultures I proceeded to culture autoclaved bion mixtures 6ac. The bion mixtures were made as described in the first report, but with the difference that the coal was first finely titurated, then heated in KCl and finally added to the mixture. Two test tubes were filled with this mixture and autoclaved at 120°C for 1/2 hour. After 24 hours one of the test tubes was used to inoculate a bouillon and an agar medium. Both gave growths, the bouillon being stronger than that on agar.

The 1st bouillon culture generation of the autoclaved bion mixture was now re-inoculated in bouillon and agar, egg medium, and blood-agar. Each gave a growth of mobile rods, cocci and amoeboid forms. At the same time an agar medium was inoculated from the 1st bouillon generation. This gave, after 24 hours, a thick yellow growth which, to my greatest surprise, proved microscopically to be a pure culture of packet-like amoeboids. At a magnification of 3700x the amoeboids showed a vigorous vibration of the internal particles, moved slowly from place to place, and at the same time showed expansion and contraction movements. The 2nd generation thus obtained was re-inoculated on agar and gave a thick growth, similar both macroscopically and microscopically. The high degree of organisation of the amoeboid forms was very distinct. Division of these forms was repeatedly and distinctly seen at a magnification of 3000x.

After the culturability of the 1/2 hour sterilised bion mixture and the 1st culture of the 1st autoclaved bion mixture was confirmed, my assistants took over the task of independently making bion mixtures and cultures. They succeeded in making immediate cultures of the same kind from eight successive 6cb bion mixtures. The bion mixtures 6cb IX, 6cb X and 6cb XI were at the same time sent to Prof. du Teil in Nice for control inoculation. We did not succeed in culturing the 6cb IX preparation, but the X and XI were just as quick as the six preceeding preparations. At the same time I told du Teil that IX would probably not succeed for it did not look good microscopically. We received from du Teil the information that X and XI succeeded and IX did not. The succeeding mixtures of 6cb XII-XX did not give a single culture. We could not explain the reason for the failure. The mixtures were made in exactly the same way as previously and yet there was no culture, as opposed to the series of previous cultures.

After much thought we hit on the idea of testing the preparations electrically, which we had not done for some time. We found that all the mixtures that had succeeded had a negative charge, while all those that had failed were neutral. From now on those mixtures that showed no electrical charge were not inoculated, in order not to prejudice the statistics unnecessarily. In a series of mixtures that had given no cultures successively, we found one which did give a culture. It was found to be negatively charged. Thus it seemed proved that electrical charge of the mixture is a necessary prerequisite for culturability.

While the fresh bion mixtures showed either anode migration or neutrality, the 2nd generation on agar (yellow "packet-amoebae") showed a very strong kathode migration, the nature and cause of which we have not so far been able to explain.

The yellow packet-amoebae (stock 6ac I) were carried through on agar to the 53rd generation. (The expression "amoebae" is not quite correct according to the ordinary usage of the term. It is justified here by the assumption that real Amoebae, e.g. A. limax, etc., consist of no more than collections of vesicles.) As they began to lose their form and yellow colour they were re-inoculated on blood-agar and egg medium. They regained their original structure and culture colour. This form has not yet been repeated with other 6-mixtures (Illus. 45).

The written reports to Prof. du Teil give some information on individual problems of culture.

In the course of time we gained experience in bion culture. We learned many ways of ensuring that the culture succeeded with complete sterilisation:

1. The materials were autoclaved or dry-sterilised individuall and individually inoculated in bouillon. The single inoculations should show no growth, this being the test of sterility.

2. The materials were mixed as described and inoculated in bouillon after each addition. These controls should show no growth.

3. The complete mixture was tested electrically. It must be strongly charged.

4. It stood for 3-4 days in a thermostat. It must be both

colloidally and microscopically very mobile.

5. From one test tube we made two inoculations in bouillon, two on egg medium E, two on agar, and one on blood-agar.

6. Successful bouillon cultures were re-inoculated on agar and on egg medium E.

Thus many prerequisites for success were fulfilled.

THE ELECTRICAL EXAMINATION.

The report deals only with nineteen bion preparations 6. Preparations of 6cb XX are dealt with elsewhere.

They bring in the question of the factors which condition the absence of electrical charge in the mixtures. Experiments had so far given no answer. It was only clear that culturability presupposed the electrical charge of the bions.

Experimental apparatus. The current was led through a rectifier with built-in transformer, choke and resistance. Thus any desired current from 0-5 mA and 0-50 mA could be used. More exact control of the direction of migration could be obtained by changing the poles by means of a current-reverser.

The ordinary slides used in these experiments were fitted with a shallow glass ring with an internal diameter of 1 1/2 cm cemented on with Canada balsam. The balsam was applied hot and in a fluid condition and, after drying, the excess was cleaned off with xylol.

Two plain platinum electrodes had been introduced through the balsam and each projected 2 mm. into the chamber. The rectified current was introduced through the platinum wires of an electrode-stand running through glass tubes.

Dark ground 400x was used, with a binocular Reichert "Z" microscope with an inclined tube.

General experimental arrangements. In all cases except where otherwise stated the current used was 2 mA. The substance was immersed in normal saline (0.9%). A few drops of this were introduced into the chamber so as to evenly cover the bottom.

Before each closing of the circuit careful check was made as to whether there were any fluid currents in the preparation from thermal or mechanical causes. It was necessary to avoid having too much fluid in the chamber, as this may lead to very disturbing currents.

When the field is free of movement the circuit is closed and it is determined whether, and to which pole, the particles migrate. On interrupting the circuit, check is again made as to whether any fluid current is seen. Finally, the current is reversed and it is determined whether the particles reverse their direction of migration.

If numerous repetitions bring consistent results, the cataphoretic nature of the migration is taken as being confirmed. It must be stressed that these examinations were not quantitative, but served only to determine the direction of migration of the substances under test.

I. ELECTRICAL EXAMINATION OF SINGLE SUBSTANCES.

1. A small drop of white of egg in about 10 c.c. of a mixture of equal parts of N/10 KCl and Ringer's solution was autoclaved at 120°C for 1/2 hour and then kept for 48 hours in a thermostat at 37°C. On testing, there was no cataphoresis. There were only quite weak contraction-movements to be seen in this preparation. Protein treated in this way also showed no cataphoresis. Inoculations were negative, i.e. no culture.

2. Lecithin, treated and examined as for 1, on the contrary, reacted to the current, even if weakly. The typical lecithin figures and vesicles which had arisen during the autoclaving migrated very slowly to the anode. Inoculations on bouillon and agar gave a culture with the same reaction.

3. Cholesterol was ground up in a mortar with KCl and Ringer's solution and then treated as for 1 and 2. It showed no change of structure after autoclaving, the particles did not migrate and there was no culture.

4 and 5. Cow's milk, undiluted, autoclaved under the same conditions and kept in a thermostat at 37°C for 24 or 48 hours; and also milk diluted with KCl and Ringer's solution, exhibited fairly strong migration of the numerous fat globules to the anode. Several cultures were positive. The culture-forms likewise migrated to the anode.

6. Red gelatine, dissolved in KCl and Ringer's solution and autoclaved, behaved as did the white of egg (1); contractile movements in the preparation, no cataphoresis, no culture.

Notes on Experiments 1-6.

Only those substances that showed cataphoresis after the above treatment later gave cultures on inoculation. The substances which had apparently the strongest charge (the greatest migration-speed) gave the quickest and strongest and also macroscopically the most distinctly recognisable growth. Cultures from the more electrically weakly-reacting preparations grew more slowly and weakly.

II. EXAMINATION OF MIXED SUBSTANCES.

7. Garden earth. For the most part the earth crystals lost their typical structure during autoclaving and broke up into small vesicles. With open circuit these vesicles moved, some of them with a fairly great speed, irregularly across the field. On closing the circuit they arranged themselves in comparatively fine rows and migrated very slowly to the kathode. On reversing the current the direction of migration was reversed. Non-autoclaved earth particles did not migrate to the kathode. The current was now passed for 1 hour at 5 mA. After this galvanising it was seen that the particles had now assumed another direction of migration, i.e. to the anode. Half an hour's uninterrupted observation with the same current and direction showed that the particles maintain this migration to the anode. All repetitions of this experiment gave the same result. Culture experiments were positive.

8. Garden earth, dry-sterilised at 180°C for 2 1/2 hours, then boiled in KCl, exhibited kathode migration. Particles had various speeds, but the movement on the whole was regular and certain. This preparation was likewise subjected to the current for 1 hour, but at 10 mA. Here also anode migration was brought about. In many repetitions of this experiment there was only once a divergent result: immediate anode migration. Since it was an isolated case the reasons for this divergence could not be discovered.

9. Ordinary gas coke, or even cinders, were powered, mixed with KCl and Ringer's solution, autoclaved, and kept for 48 hours in a thermostat at 37°C. Even if only to a very slight degree, the coke broke up into small vesicles which had the same characters as those from the earth.

10. Incandescent coal. Powdered coal was put on a spatula and heated to redness in a flame and then put immediately into sterile KCl solution. In sterile drops of this mixture there was seen the same independent and cataphoretic movements as in autoclaved coal-KCl-Ringer's mixture. Galvanising of both coal preparations transformed the kathode migration into anode migration as in the earth preparations.

Preparations 8-10 gave cultures in bouillon and on agar.

Notes on Experiments 7-10.

Microscopic examination during the galvanising had to be given up because of the vigorous formation of bubbles of gas, due to the strength of current employed (5 & 10 mA). Samples were continually removed from the galvanised preparation and subjected to a current of 2 mA under the microscope, when the phenomena found in 7 were observed. Also with the heterogeneous substances here examined there was seen the same connection between electrical charge, demonstrated by the occurrence of cataphoresis, and culturability, as in 1-6. In the isolated cases in which there was no migration - this was somewhat more frequently the case with the particularly non-homogeneous dust - the culture attempts were also negativ

III. EXAMINATION OF PREPARATION 6.

The following survey shows how also with these preparations there was a correspondence between electrical properties and culturability.

11. 6c I. This preparation (autoclaved) showed anode migratio This was used to inoculate a medium which consisted of the same substances as the preparation, stirred up with protein to form a thick mass. This medium was next sterilised and fo control was kept for 48 hours in a thermostat. The culture succeeded. It consisted of two kinds of growths, one hard an one soft and slimy. Each culture-form was separately and successfully transplanted on to agar. The microscope showed the soft slimy culture to consist of typical "packet-amoebae". The hard culture consisted exclusively of vesicles. Electrical examination showed the "packet-amoebae" to have kathode migration. Galvanising reversed this direction of migration. The vesicles, on the contrary, migrated as did th particles of the preparation with uniform speed to the anode.

N.B. The uniform cataphoresis is better observed in the culture than in the original preparation. Now, since the

cataphoretic preparations give cultures, it seems possible to conclude that the forms of the culture arise from those of the preparation which migrate in the electrical field. In the cultures, then, the cataphoresis must naturally be more complete and therefore more distinct in appearance.

12. 6c II. In the preparation there were likewise the bion forms (vesicles). They migrated to the anode. The culture, which was fairly hard, was also cataphoretic. There was no soft growth.

13 & 14. 6c III & 6c IV. These showed the same state of affairs.

15. N.B. From here on the following procedure was used: finely powered coal was dry-sterilised at 180°C; lecithin, cholesterol, gelatine, white of egg, yolk of egg, and KCl and Ringer's solution were autoclaved. The sterile mixture of these substances was then heated in the steriliser at 180°C until very opaque.

6cb IV. A test taken from the distinctly opaque preparation immediately after heating showed the bion picture immediately under the microscope. The vessel was opened several times to make inoculations. Later, when tested electrically, the preparation was seen to be very impure. Besides single vesicles there were also heaps of vesicles which reminded one strongly of fermenting cultures, without it being possible to say that they were connected with fermentation. Later we saw in the preparation a number of thread-bacteria which seemed to be completely lifeless. The greater part of the preparation seemed to consist, apart from the vesicles and heaps of vesicles, of rods of various sizes showing very slow and faint movement.

Most of the forms of this preparation migrated to the anode. But there were also some rods which migrated to the kathode. After a short time these rods seemed to become re-charged; one moment they exhibited strong quivering movements and then moved towards the anode, they soon became positive again and moved towards the kathode. The long and apparently lifeless thread-bacteria did not migrate. The culture made from the not-yet contaminated preparation straight after removal from the thermostat, consisted of the same micro-organisms as the earlier cultures of mixtures of the same substances and showed the same cataphoretic appearances.

16. 6cb VI. This gave a very beautiful picture of vesicles and vesicle heaps. All the vesicles migrated with uniform speed

to the anode; likewise, but more slowly, the small vesicle heaps. The larger vesicle heaps showed no sort of migration with this strength of current. Inoculation gave a pure culture consisting of vesicles and vesicle heaps with clear and strong migration to the anode.

17 & 18. 6cb VII & 6cb VIII. These gave the same picture, both for the preparation and for the culture.

19. 6cb IX. This showed the same forms as the previous ones, but made a much less vigorous impression and showed no migration in very numerous trials. On closing the circuit there were fairly strong contraction movements but no cataphoresis. Cultures were negative, even after 6 weeks there was no growth.

20. 6cb X. This preparation had the same appearance as the others. The migration phenomena were less distinct than in 17-19. There was a general anode migration, but isolated particles moved transversely to the current direction. The culture succeeded and had the same properties as the cultures 17-19.

21. 6cb XI. The migration was more distinct, the culture was as above.

22 & 23. 6cb XII & 6cb XIII. The protein was here replaced by autoclaved meat. Under the microscope quantities of destroyed muscle fibre were to be seen. Cataphoresis did not occur, cultures were negative.

24-27. 6cb XIV-XVII. Protein was again employed and the meat omitted. Cataphoresis and culture were negative.

28 & 29. 6cb XVIII & XIX. These showed slight signs of electrical charge - less distinct cataphoresis - which indicated a more amphoteric character. Cultures were negative.

Notes on Experiments 11-29.

Preparations without electrical reaction gave no culture. If the cataphoresis was strong, growths showed after 24-48 hours, while if weak, growths did not generally develop until after 8-10 days. The migration phenomena of the cultures corresponded to those of the original preparations.

The results of the differentiation examinations (biological staining of different sorts, comparison with known microbes, mutual interaction, cultures of galvanised bions, employment of the customary bacteriological methods on the various bion cultures, etc.), which have not yet been concluded, will be published later.

Chapter IV.

COMMENCEMENT OF THE CONTROL WORK BY

PROF. ROGER DU TEIL.

I shall interrupt my account and interpolate my communications to Professor Roger du Teil, his lectures and parts of our correspondence. They clarify important details and the first uncertainties of January-April 1937, and also obviate the complication of my self-contained account.

COMMUNICATION ON 7th MARCH 1937

BY PROF. ROGER DU TEIL

TO THE NATURAL PHILOSOPHY SOCIETY IN NICE

ON THE WORK OF DR. REICH (OSLO).

Dr. Wilhelm Reich, a pupil of Freud, who began by specialising in the field of psycho-analysis, has devoted himself since settling in Oslo (Norway) to laboratory work connected with his special subject. After having discovered the electrical charge of the surface of the erogenous zones of the human body, he observed tension-changes which take place in these zones accompanying certain sensations and feelings, particularly joy, sorrow and anxiety. To this end he had constructed a tension-meter apparatus which consists essentially of an electron tube connected to a circuit and an oscillograph. The ~~tracks recorded~~ paths traced on a film by the oscillograph beam form a direct expression of the significance and the dimensions of both the oscillations and the corresponding feelings. Thus I can show you here the curve of pleasurable sensations called forth by, for instance, tickling the hand (tickling-phenomena) - here that of the flavour of sugar on the tongue - this unpleasurable one is caused by the taste of salt - this shows the pleasurable sensation of two people during a warm handshake - this one during a kiss. And here, after the kiss of a happy couple, I even have that of a couple whose happiness is fading, since the feminine partner appears not to enjoy the kiss sufficiently. You can easily observe the sinking of the curve.

I shall not here go into the practical interest - and

a humourist (we are in France) would doubtless say the danger - of such a discovery. I only mention it in order to give you a picture of Dr. Reich, who is a laboratory worker, a true scientist, who has been conversant with experimental work for a decade and is consequently accustomed to employ the experimental method in all its strictness. I shall name you some of the works which Dr. Reich has had published just as they occur to me:

Character-Analysis and its Technique.
Dialectical Materialism and Psycho-Analysis.
Psychic Contact and Vegetative Currents.

as well as other works dealing with mass-psychology and its relation to psycho-analysis.

These introductory remarks are no waste of time. We shall in fact test and judge - insofar as a fragmentary process can be judged on the basis of MSS - a series of experiments of which it must at least be said that their results are absolutely astonishing and seem to contradict the most solidly founded scientific dogmas. A judgement of this kind requires, indeed demands or better would demand, absolute objectivity. I say "would demand", for absolute objectivity is not of this world - and perhaps of none - and it is entirely natural that we approach the examination of such a question with certain feelings. These feelings are all the stronger because this question casts on the dust-heap knowledge which, since we acquired it and looked upon it as final, has become part of ourselves. Hence the atmosphere which surrounds such a communication is of great significance. Not that one expects from it a prejudice in favour of the views to be discussed, but rather the elimination of any prepossessions in the interest of that impartial objectivity whose desirability I have expressed. In fact Wilhelm Reich's past and his works entitle him to demand from us this objectivity, the rejection of bias, which Descartes condemned - as well, by the way, as over-hastiness - and a sympathetic, that is a scientific curiosity, to which belongs the most sharply critical attitude.

Dr. Reich's guiding idea, which already appeared in his earliest experiments, consists in the identification of the electrical tension-charge process with the process of vegetative life. In this field and guided by this idea he has for several years been carrying out investigations into the origin of life with the said process as starting-point. These investigations have led him to make experiments in the production of artificial unicellular structures in which the

phenomena of vegetative life are brought about by an exclusively electro-chemical process. Naturally, he introduces no independent electric force into the elements he combines. In the colloid realm, in which he is working, the phenomena of surface tension and molecular movement together with electricity arise spontaneously, at least in the form of movement. And movement is regarded as one of the characteristics of life.

He has thus had to make it a basic principle to carry out his experiments with sterilised materials, i.e. those from which according to our present means and knowledge every previous dynamism properly described as living has been eliminated. In other words; using the technique of radical pasteurisation he proves that, in fact, there is no radical pasteurisation. Here I should like to make some incidental remarks.

When? 1935 and 1936 start

In the course of a visit made for this purpose Dr. Reich had personally informed me concerning his work. He had left me some of his papers for translation, in order to publish them in France. But I had heard nothing further of these detailed experiments for a year, when on the 8th January of this year (1937) he sent me with a letter and a cursory report two sealed and sterilised ampoules, which he asked me to examine with a microscope of approximately 3000x magnification. I undertook these examinations almost immediately and will read you the report which I drew up directly after observation. First however I should like to acquaint you with the report from Dr. Reich which provided the basis for the work. I shall read it in translation. It is a hasty translation whose style you must excuse, for I have not had time to correct it. I can in any case guarantee its accuracy, even if it is lacking in elegance. Furthermore, the original of this report, as of all the papers whose translations I shall read you, is here to hand and at the disposal of anyone of you who can read German and feels the entirely justified wish to know the original text.

On the 8th January 1937 I received the following report:

Provisional Communication

on the production of life-like formations

on the basis of the tension-charge formula.

(The text is to be found on p. )

Of the reports so far received I made the following summary:

Report.

In a letter of the 8th January, Dr. Wilhelm Reich of Oslo sent me a provisional communiqué relating to the construction of formations possessing the characteristics of living organisms, on the basis of the tension-charge formula.

According to this communication Dr. Reich has, after several years' research, obtained through purely physical and chemical processes formations showing all the characteristics of living organisms. The report details the experimental process, i.e. the constituent elements of the formations, as well as the order in which the elements were applied. This order, in addition, seemed to the experimenter to be the most important basis of the experiment.

Sealed ampoules of about 5 c.c. accompanied the report. They bore the following label: Bions 6b. Sterile. 12/1/37. A brief note accompanying the ampoules stated: to be observed at a binocular magnification of 2000x-3000x.

On Tuesday 26th January, i.e. 12 days after the date of production stated on the ampoules, I was able to make a series of observations on the contents, conforming to the conditions requested by the experimenter; thanks to the assistance of Drs. Ronchese and Saraille who possess in Nice an analytical laboratory with the most up to date equipment. I was able, in fact, to make use of a binocular microscope which magnified at least 2500x and was capable of 3000x, a magnification which, in spite of difficulties in illumination still permitted fruitful observation.

After one of the ampoules had been opened a specimen was at once placed on a sterile slide. A second slide was immediately placed over the first and sealed with paraffin, in order to prevent any evaporation and any tendency of the fluid to collect at the edges which accompanies evaporation.

Observation showed immediately and with incontestible exactness the appearances described in Dr. Reich's communication. I was able to observe in such manner four main types of formation:

1. Rods, which at a magnification of 2500x had apparent dimensions of 1/2-1 cm., i.e. were absolutely observable.

These formations moved in two different ways. Sometimes they were seen undulating energetically forward longitudinally with sudden stops and starts, in a manner similar to what one describes as crawling, i.e. they make the impression of fragile bodies, fairly wide and flat and reminiscent in shape of certain fish. In swimming they show themselves now sideways on, now from either of the flat surfaces, and seem to move forwards, sideways or up and down by means of undulations just like fish in an aquarium. These formations have a nucleus which is itself animated by movements and vibrations. In addition, they are subject to division and in this behave in every respect analogously to living cells. I have observed several divide before my eyes to produce two similar formations, which retained the recognisable characteristic shapes.

2. Uni-cellular formations shaped like fungi, with a luminous nucleus animated by continuous tremors.

3. Considerably larger formations, with the appearance of mycelia, having spores at the termination of each branch. These formations are stationary, but are animated by a continual, almost imperceptible and very slow expansion and contraction. Their dimensions are enormous in comparison with the foregoing. At 3000x magnification their apparent length is several centimetres.

4. Undivided, anucleate cells which move in an obviously mechanical manner as though controlled from without.

The preparation as a whole had absolutely the appearance of containing living germs, which contradicted a priori the precise declaration that it had been obtained by boiling and was absolutely sterile.

Nevertheless, it seems to us that the main experiment that would decide the question of the living or non-living state of these formations would be attempts at culturing. Only the progressive elimination of the artificially formed primary elements, and their substitution by new formations produced without outside aid would reveal whether we are dealing with a dynamism belonging to the realm of the living, or a mere appearance "imitated" by chemical and electrical processes. It is to be hoped that the experiments will be carried forward to the final clarification of this fundamental problem. In any case, honour is due to Dr. Reich for having proceeded thus far.

Nice, 3rd February 1937 Roger du Teil.

I must add to this report that Dr. Rochese, who examined the cell-formations in question with me - in fact, before me - after reading Dr. Reich's report very carefully, did not for a moment doubt the correctness of the experiments which produced the bions, though he reserved judgement on the possible interpretation of the phenomena observed. This attitude on the part of a bacteriologist must be mentioned here, since it throws a favourable light on that correctness, that seriousness, which must rule all researches of this kind. And I should like to add at once that M. Deal, the bacteriologist from Cannes, who has also read the various reports and handled the cultures, though he too made reservations respecting the interpretation - incidentally, different ones to M. Rochese - also had not a moment's doubt regarding the concrete facts, that is, the development of germs which had been two hours in the steriliser at 180° C. He even added that the culture he had handled had all the appearances of a "pure" culture which would exclude the hypothesis that we are dealing with so-called "air-germs". We shall see further that in the later experiments particular care was taken to eliminate these various criticisms.

On the 8th February Dr. Reich wrote to me afresh and informed me that he had methodically cultured bions and that he would keep me au courant. Like myself, he was of the opinion, which I had communicated to him, that the success of these cultures would be of the utmost importance for the possible interpretation of his discovery.

On the 16th February Dr. Reich, in reply to a request of mine, sent me all the details necessary for me to set down a bion culture myself. Thus, he did not avoid a control, but on the contrary sought it. This is another favourable indication of his certainty of having made no experimental error. I shall now read you his letter.

Oslo, 16th February 1937

Dear Professor du Teil,

I should like to communicate to you some useful and definite results of my attempts to culture bions. I was entirely in agreement with you that the question of spontaneous generation cannot be decided by microscopic findings, but essentially only by successful cultures of sterile, i.e. boiled, colloid mixtures. I am glad to be now able to report to you some positive results. The matter is simpler than it appeared to me in these months of experimental work.

I should be extremely grateful if you would, as you wrote, repeat and control my experiments in a French laboratory. I am convinced that this could only be of advantage for me. I am therefore heartily in agreement with your desire to repeat the experiments, and am glad to hear of it. The same applies to the cultures.

In recent weeks I have succeeded in making cultures of fresh bion-preparations, including all four types, on agar media and in bouillon. It has proved that quite freshly-boiled bion mixtures grow up much more slowly and give far less movement in the culture than boiled bions 2-5 days old. I would recommend you to prepare such a bion mixture yourself from the instructions contained in my first communication, to boil the mixture itself at 160°C in a dry-steriliser for about an hour, and first to let the paraffined and sealed ampoules stand for 3-4 days. From the sterile ampoule about two drops are taken sterile with a Pasteur pipette and smeared over the surface of the agar medium. After 24 or 48 hours arises either a fine, corrugated covering, neutral in colour, or a dense, grey-white growth. I have not yet been able to establish what determines this difference. Of much clearer significance is culturing in bouillon. Already after 24 hours the bouillon clouds strongly and shows in the microscope an extremely mobile picture of rods, round cocci, large nucleate cells, and finally, internally mobile amoeboid formations.

Yesterday I boiled a bouillon culture of bions for a quarter of an hour in the steriliser, but at a magnification of even 250x in the dark field both the movement and shape of the formations could be observed. I cannot believe my own eyes when I see this, but we have made the culture experiment in the most varied ways so often that doubt is no longer possible.

I am enclosing a culture specimen. Should you be convinced of the correctness of my assertions I beg you to inform the Academy. I shall now complete the control experiments myself, prepare a detailed communication and send it to the Academy and a copy to you.

W. R.

19.2.37. postscript:

I hesitated some days over the despatch of this letter, because the control experiments produced very remarkable results and revealed very remarkable facts. But it seems it

hour was not sufficient to exclude infection by bacterial germs from within or without. In order to test this objection we shall now make the following experiment. We shall now sterilise the dry substances which comprise the bions in the dry-steriliser for 2 hours at 180°C. The fluid necessary for bion production we shall sterilise for 1/2 hour in the autoclave at 120°C. We shall then mix the dry and the fluid substances, keep this mixture sterile and after 48 hours inoculate it into the bouillon. As a second control experiment we shall first mix the substances and then autoclave the mixture at 120°C.

The following additional remarks seem important to me. The aim of these experiments is not to prove that bions move like living beings, for that can be definitely established microscopically. These experiments have the sole object of eliminating the objection that the culture findings are a matter of infection from without by the so-called germs in the air. Furthermore, I am also making simultaneous experiments with the inoculation of dust from the vacuum cleaner in the most various forms, in order to test the correctness of this objection. So far it has proved that the dust cultures have different appearance to the bion cultures both macroscopically and microscopically.

I am writing this for your provisional information and to assure you that we are employing here every conceivable care to substantiate the culturability of bions as a finding of unequivocal significance; with full scepticism, but also with absolute readiness to guarantee what has been found.

With best wishes,

Yours,

W. R.

The cultures announced did not arrive at once. Dr. Reich telegraphed on the 25th February to inform me of their success and despatch.

On the 27th February I finally received one batch of cultures, which was followed in 3 days by a second, accompanied by a schema, a letter and a second report.

Oslo, 27th February 1937

Dear Professor du Teil,

As I told you in my telegram last week, the autoclaving experiment was successful. I send you herewith specimens of cultures from autoclaved mixtures, as well as a second provisional communication concerning the positive result only. You will find with the cultures a schema on the inoculation of cultures as well as a description in the communication.

May I ask you to select and forward some of the cultures to the Paris Academy as well as the second copy of the "Provisional Communication". I beg you to let me know whether you were successful in producing and culturing bions according to the instructions in my letter. I should particularly like to draw your attention to the remarkable agar-amoeba-forms, which have greatly astonished me.

With hearty thanks,

I remain,

Yours,

W. R.

Second Provisional Communication

relating to the culturability of bions.

by Dr. Wilhelm Reich (Oslo).

Here follows verbatim the report on the culturing of bions (preparation 6) on bouillon and agar, together with positive autoclaving results, as in the chapter on culturing (Chapter III, p. ).

In addition, you have here the cultures we are discussing and can examine them, at least macroscopically. I may inform you that the culture on agar, which tends to assume this wavelike shape, has developed very noticeably during the few days it has been in my possession; it grows as one watches. If one consider that we are dealing with autoclaved which have been at 120°C one is amazed - and a little dubious regarding the effectiveness of current methods of sterilisation. I say this without meaning any offence to our

respected member from the pharmaceutical branch or our honoured medical members.

I have finished the relation of the objective facts. Before I hand their discussion over to you, I should like to express some personal views on them.

The conclusions which one can draw from these experiments are of two kinds. They may relate either to the facts or to their interpretation.

The facts themselves can be looked at from two points of view, relating either to the construction of organised formations out of unorganised matter, or to the resistance of these formations to destruction by sterilisation. Let us add that both these series of facts have common ground in the fact that the unorganised elements employed in the experiment are previously sterilised and that a fresh sterilisation is carried out at every stage of the experiment, so that the question of the potentialities of the unorganised material arises afresh at every stage. This circumstance, furthermore, permits a multiple testing of the findings, since with each boiling a completely new experiment on the problem of transition from unorganised to organised matter is carried out. In fact, to the enquiries which I made of specialists - and I beg the specialists present to confirm or deny this - I received the reply that present-day biological knowledge knows no germs which withstand a temperature of 180°C. The minimum achievement of Dr. Reich's discovery would be therefore (if I may say so) to have found organisms showing all the characteristics of life and resistant to these temperatures. And this discovery can from now on, with the help of the bions and bion cultures sent to me by Dr. Reich, be confirmed by everyone of us.

While we are on the subject of the facts and their interpretation, there is another series of viewpoints which can be adopted in relation to the question of the endogenous or exogenous origin of the germs thus discovered.

To the remark that it might be a matter of bacteria from the air, Dr. Reich replies, as you have heard, in two ways. First, as to the other objections, by the sterilisation at 180°C, which no known germ resists; secondly, by taking atmospheric dust from the vacuum cleaner and culturing it. The cultures obtained have nothing in common with the cultures which L. Deel yesterday characterised as obviously "pure" cultures.

To the objection that the culture medium might contain germs, the first answer is that their development would not be confined to the inoculated area. The second consists again in the sterilisation. The third in the fact that such germs would be polymorphous and would also not present the picture of a "pure" culture. Now that such a favourable light has been cast upon the facts we will come, in expectation of an absolute confirmation, which will not be long delayed, to their interpretation. Here we encounter two objections to Dr. Reich's hypothesis - though he has laid no emphasis on this point - that we are really dealing with living organisms.

The first objection is that we are only dealing with electrical and chemical processes exhibiting a movement related to Brownian movement (this is Dr. Konchese's principl objection). Reich answers it with the culture.

The second objection, raised by M. Deel, is that lecith is a living substance. The discovery is, therefore, not that of the missing link in the chain from inorganic to organic, but only consists in "having organised" a living, but not yet organised, material. The answer to this objection is that lecithin in yolk of egg is only regarded as nutriment, while life is probably localised in the embryo. The process of obtaining lecithin - agitating and dissolving in ether, purifying with zinc chloride, which forms a soluble double salt from which one regenerates the lecithin with hydrogen sulphide - gives entirely the impression of an exclusively chemical process devoid of any kind of vital dynamism. And then the sterilisation, at least according to the present state of our knowledge, would in any case exclude life. Even if lecithin were something other than a chemical constituent of yolk of egg it would be killed by this sterilisation.

If, finally, it were objected that we are dealing with electro-chemical processes which completely and absolutely imitate in all its expressions what we call life - we would have to reply without any doubt: if two triangles are placed one upon the other, which is then the first, which the second? Which life would be the original and which the imitation?

I should like to close this communication, which already offers matter for discussion from the scientific standpoint, with a brief glance at the metaphysical realm. Once it is admitted that we can construct by electro-chemical processes unicellular organisms with all the properties of life in the irreducible sense in which we have hitherto understood it,

shall we not see the fundamental quality of life, no longer in these material phenomena of movement, nourishment and division, but in the organisation of differentiated formation springing from one germ, which corresponds to one type, the species? And would not the result of this experiment, which seemed a priori to dispose us to a materialistic solution of the problem of life, consist on the contrary in forcing us to see life in an "organising intention", to assign it radically to the realm of spirit?

Practically speaking, I believe that our intervention and the interest that we bring to this discovery can and must be of great service to science, since they will make it possible for Dr. Reich to announce his discovery in France at the Académie des Sciences, if we recognise the correctness of his experiments and bring to his notice errors in the experiments or the interpretation, in case we find any. And in either case we shall have done a useful work.

I should, therefore, like to ask you, after the discussion, to elect some of our members to the task of assisting me in the series of experiments which I intend to undertake for the purpose of confirming the results of Dr. Reich's work.

Signed, Roger du Teil.

Addendum:

After a discussion had taken place, the Philosophical Society, recognising the obvious interest of Dr. Reich's work, whatever its interpretation, elected the following members as assistants to M. Roger du Teil in his control work:

Dr. Chartier.

Dr. Perisson,

Mlle. Fermand, Professor of Natural Sciences.

M. Claude Saulnier, Pharmacist.

In addition, it expressed the wish that this work should at an early date be entrusted to a big French laboratory, preferably the Lumière Laboratory in Lyons.

Meeting of Sunday, 7th March 1937.

Chapter V.

CULTURE EXPERIMENTS WITH EARTH, COKE AND SOOT.

1. EXCLUSION OF THE POSSIBLE OBJECTIONS REGARDING PRE-EXISTING SPORES.

The control experiments, which for some two years have been carried out contemporaneously with the bion experiments, derived from two fundamental questions of interpretation:

First: Could the bions and their cultures be the result of an infection from the air?

Second: What specific significance have the individual materials from which the bions are formed by boiling?

The first question can be easily answered by the following facts: the bions cannot arise through infection from the air during their production, for:

1. The mobile cocci, rods and amoeboid formations are present from the moment when the first and second mixtures are combined (cf. first provisional communication).

2. Infection from the air is excluded by very careful application of sterilisation technique.

3. The bions also do not lose their motility and culturability through long and repeated boiling and autoclaving.

The sterile bions and bion cultures of mixture 6 differ macroscopically and microscopically from rot bacteria, mould fungi and other growths found in unsterile fibre or grass preparations in the form, movement and colour of the microscopic formation.

It is different with the second question: Could it be that the bions do not arise from the unorganised, inanimate materials which are boiled together at all, but from "spores" which might be found in materials like charcoal and gelatine and be resistant to boiling at 100° or 120°? In this case we should be dealing not with the organisation of inanimate matter into animate formations, but merely with the development of unknown spores.

In the course of the work on boiled coke and earth, the first observation, that these substances exhibit far more movement or "life" in a boiled than in an unboiled state, was confirmed again and again. The objection that boiling released spores already present in the material thus repeatedly presented itself, not only in my own thoughts on the matter, but also in those of bacteriologists. This was said to be nothing particular. There were merely spores present in the materials which were released by boiling. It was now clear that simple observation of the boiling experiment and comparison with unboiled substances were not sufficient to finally refute this conception of pre-existent spores. I did not doubt for a moment that there are spores from which living beings can develop. But the assertion that these spores were present from eternity contradicted all my observations. Many considerations spoke against it. How could the spores have entered into coke which has been roasted at thousands of degrees and from which all other substances have been distilled? How could spores appear in grass or in structured striated muscle, where in an unboiled condition of the substances they were not visible by the highest microscopic magnification? And how could they suddenly appear after muscle, moss, grass coke or earth had been boiled? As strongly as these considerations refuted the spore theory, the refutation remained theoretical. It had no experimental basis. I was now faced with the task of finding a system of experiments which would clearly establish the facts. What I shall describe in the following pages might give the impression that the work proceeded smoothly, without difficulties or worries. The contrary is the case. There were weeks and months during which the work was at a standstill, as though stuck in a cul de sac before a simple way out occurred to me.

In the previous boiling experiments the processes of sterilisation and immersion of the material were carried out in one. It now occurred to me that the sterilising, i.e. the destruction of the existing spores, and the life-begetting immersion could be separated from each other.

Earth and coal (separately) were pulverised, put into Petri dishes and placed in the dry-steriliser at 180°C. Two hours was maintained as the standard time of sterilisation at 180°, but shorter and longer periods of dry-sterilisation were also tried with the same results. Before the termination of the two hours, autoclaved 0.1 n. KCl was measured out into sterile test tubes so that these were two-thirds full, and then tightly closed with water-proof cotton-wool. The KCl, which had been previously sterilised, was then autoclaved again for half an hour at 120° as an added precaution. A glass or metal

spatula was then slowly heated to redness several times in a gas flame. The dry-steriliser and a test tube of KCl were opened. As quickly as possible a spatula tip-full of coke dust or earth dust was tipped into the test tube of KCl. This was immediately closed and placed in the steriliser. Alternatively, the earth or coke can be sterilised dry in test tubes and the KCl added after two hours. The test tubes were placed at an angle to give a greater liquid surface. Experience showed that this made easier the dissolution of the earth or coke dust. The KCl-coke or KCl-earth mixtures was boiled like this; definite periods of time for the boiling could not be fixed, since it was not a question of the time of boiling, but that, as a result of the boiling, both the earth and the coke mixtures took on a dense, turbid, colloidal character. This took place in some cases after half an hour's boiling, in others only after one or 1 1/2 hours', and depended on the quantity and size of the particles of the earth or coke.

The most important factor in the whole procedure is the attainment, even if only for a short time, of a stable condition of the coke or earth particles in suspension in the fluid. The experiment gives no positive result if:

a) the coke or earth particles are still lying on the bottom and the fluid is clear when the test tube is taken out of the dry-steriliser;

b) suspended coke or earth particles sink quickly or slowly after being taken out of the dry-steriliser.

A positive result can only be achieved if suspension of the particles is maintained, if only for 5 minutes. The particles usually sink sooner or later in well-boiled preparations.

A few drops were taken with a Pasteur pipette from the coke or earth suspension, irrespective of whether or not it was in a state of boiling, and examined microscopically and electrically. Two things were shown: Electrically, both the coke and the earth particles moved to the kathode on transmission of a current of 2 mA through the preparation. If the current is reversed the direction of movement is promptly changed. Equally, movement promptly stops if the current is interrupted.

After current transmission of some length the particles become electrically negative. Exactly like, say, Staphylococ

Microscopically, we examine in two ways: First in the dark field with a 10x objective and about 16x compensating eyepiece with binocular tubes set at an angle. With correct setting of the dark field screw, in the densest central stratum of the preparation the liveliest movement of individual vesicles and heaps of vesicles can be seen. If a magnification of at least 2000x is used, a few coke or earth particles can be seen lying still, but the majority of the earth crystals are green-coloured and vesicularly disintegrated, particularly at the edges. Tubes can be seen which move, stretch, contract and bend. The vesicularly-structured coke and earth formations crawl about in the field with jerky, "clumsy" movements. Rods and cocci can be seen wandering through the field with quick, tremulous movements. If the suspension was not sufficiently good, the same formations are to be seen, but less mobile or altogether immobile. Permanent suspension of the particles in the fluid, electrical positivity, and movement clearly belong together here. If the above-named three fundamental characteristics of the preparation are present, bouillon is inoculated from the boiling colloidal solution. In many cases the earth and the coke inoculation produces a dense, milky clouding of the bouillon after 24 hours. Microscopic examination shows preponderant rod-shaped formations, moving either swiftly or slowly. The coke or earth amoeboids, which were taken from the boiling preparation on the previous day, are no longer to be found. On the other hand, unstructured crystals from the inoculation are occasionally to be seen. Now an agar culture medium was smeared with sterile precautions with a drop from the bouillon culture. After a further 24 hours, often earlier, a yellowish granular growth arose. For the control of this growth two other cultures were laid down:

1. Entirely unsterile; another agar culture medium with coke or earth dust.

2. A further agar culture medium with earth or coke dust, sterilised dry at 180°C.

The first gave an irregular growth of various colours and shapes; the second gave no growth. The agar growth of the first sterilised and then boiled coke or earth mixture differed in every respect microscopically from the unsterile agar growth. Earth and coke treated in the manner described proved to be multiple-culturable. The culturability of sterile earth and coke immediately after boiling is the more certain the more correctly the following methods of treatment, gained by experience, are adhered to.

1. The coke or earth crystals must first be finely pulverised mechanically.

2. The evaporating KCl solution is made up by the addition of further sterile KCl.

3. The fluid must be uniformly colloidally clouded, the particles must not sink to the bottom.

4. The preponderance of crawling formations must be ascertained by microscopic examination.

5. It is best to inoculate immediately out of the boiling solution.

6. The first generation in bouillon is inoculated on agar, or better still, on egg and then on to agar.

Let us now summarise the errors which might prevent the production of coke or earth cultures.

1. If the coke or earth is dry-sterilised for too long without boiling it for a corresponding period, the vesicular disintegration of the crystals remains insufficient.

2. If care is not taken to see whether the particles sink after being taken out of the steriliser, the culture may easily fail.

3. If the test tube is taken out of the steriliser carelessly or even shaken, the substances lying on the bottom are shaken up and give the impression of suspension.

4. If the preparation, which has just been boiling, is not microscopically examined, it is impossible to tell whether mobility of the formations is strong enough.

Coke and earth, as well as moss, treated thus resulted in some cultures in bouillon. But on the whole the culture results remained uncertain; although the culture experiment was one of the most decisive measures in the whole work. If a culture did not succeed there were two possibilities: Either the substances, which had previously been entirely dried up by the dry-sterilisation, had not been boiled for long enough to attain the necessary disintegration of the particles and infusion. Or the culture medium was not sufficiently complex to provide nourishment for the life that was "flickering up" and to maintain it. On the other hand, the microscopic

finding remained always typically the same, as long as I boiled long enough for the colloidal clouding. If the preparation is left in fused ampoules for 2-3 months, cultures are often easily produced. So far the question of why one culture succeeds and not another could not be answered.

In the subsequent series of earth and coke experiments it proved that certainty of culturing increased with the application of higher temperatures and the use of the smallest particles.

## 2. THE INCANDESCENT-COKE EXPERIMENT.

I shall now proceed to a description of the incandescence experiments.

On the 21st May 1937 it occurred to me that the spore theory could be tested in a very simple, but also radical, manner. It was possible to make an experiment with coke that would leave no room whatever for the possibility of pre-existing spores. The experimental procedure, which I carried out, is very simple. The phenomena are unambiguously visible with a binocular inclined microscope giving a magnification of 2500-3000x with immersion. I will state in advance that this experiment was carried out ten times in succession and always gave the same result.

Coke dust, which had been dry-sterilised at 180°, was placed on a thin metal spatula. It was heated in a benzine gas flame at about 1500° C until the mass had reached red heat but without being reduced entirely to ash. Meanwhile, my bacteriological assistant prepared a mixture of bouillon and an equal part of previously autoclaved KCl. I proceeded in this from the assumption that in this experiment not only nutriment but also swelling-substance must be present. The incandescent coke was immediately added to the bouillon-KCl mixture. To our great astonishment, every experiment carried out showed a momentary colloidal clouding of the solution, to a degree never before attained with simple boiling.

A further phenomenon was instructive and surprising. The coke particles gave the solution a black colour, but in the course of 10 to 20 minutes the black coloration began to disappear and give place to a grey cloudy turbidity. This solution was immediately examined microscopically at 3000x, and also after 24, 48 or 72 hours. It now proved that very mobile life existed immediately after the preparation had been made up. Next a sample of incandesced coke dust was placed

dry on the microscope slide. At a magnification of 200x in the dark field the crystals could be seen disintegrating into very fine vesicles. On adding KCl, movement started. The vesicles, which had the appearance of spores, partly moved locally. The large coke particles rapidly absorbed fluid. After some 1-2 minutes the large coke particles, which were completely vesicular at their margins, could be seen drawing individual vesicles in the fluid towards themselves. The movement of the individual vesicles towards the large coke particles gradually speeded up with decreased distance. In the end the vesicles sped towards the larger coke particles and remained clinging to their margins. These were obviously electro-magnetic phenomena.

At a magnification of 3000x the incandesced coke particl could not be differentiated from living formations. They actively moved round in the field of vision. The middle and upper layers of the preparation showed considerably more movement than the stratum lying on the bottom. After 24 hours the KCl solution was full of cocci and rods which moved locall The formations proved to be positively electrically charged. The origin of the formations could be immediately recognised from their blue-black colour. The cocci were of all sizes. On the margin of the larger formations were strong vibrating movements, which persisted after the preparation had cooled and were, therefore, not heat phenomena. The first preparati gave a pronounced clouding in bouillon after 24 hours. With the others, clouding was absent and inoculation gave only a f cultures. As a basis for further work I had to draw a provisional conclusion from the phenomena observed.

In the coke particle, which has already been through a high degree of roasting, the individual particles are firmly bound together. When boiled, and particularly when incandesced this coalescence is destroyed, liberating electrical energy, for otherwise the individual particles could not be electrically charged, which the large pieces are not. These particle into which the coke disintegrates on incandescing, rapidly absorb fluid, as can be directly seen under the microscope at 2000x on the addition of KCl solution. The particles have bee transformed into vesicles which in shape are indistinguishable from spores. This experiment seemed to me to absolutely contradict the spore theory in the form in which it rules today; for no-one can maintain that spores can withstand a temperature such as is reached in incandescing. Furthermore, there was the additional fact that none of the substances I ha observed failed to take on a vesicular structure on boiling, autoclaving, infusion or incandescing. And, as stated, the

vesicles are indistinguishable in appearance from spores. I therefore do not deny the existence of spores in the sense of the old theory. I merely maintain that the vesicular character and the disintegration of infused, boiled or incandesced unorganised matter into vesicles is the central point from which further research into the formation of spores capable of developing into living forms must proceed. The spores also must arise through infusion of a substance. This appears to me an absolutely essential assumption.

Its accuracy can also be demonstrated cinematographically. Fine coke dust is first sterilised dry for two hours at 180°C. Some granules are placed in a cavity slide and autoclaved KCl solution added. The preparation is immediately sealed with paraffin around the cover glass and viewed at 300-600x with a dark field. Some 50 cm. of film are now exposed at normal speed. Then the slow-motion attachment is set at one picture per 2 or 5 hours. For the slow-motion photography it is best to select a flat, sharply-margined granule, as free as possible from vesicles. The slow-motion apparatus functions uninterruptedly for 6 to 10 weeks. Daily observation soon reveals to the eye the advance in the vesicular disintegration of the granule. In time more and more mobile vesicles enter the field. If the film is screened it shows at first a dark spot which becomes illuminated at a greater or lesser speed according to the degree of acceleration employed.

3. CULTURES FROM INCANDESCED SOOT. THE BIOLOGICAL INTERPRETATION OF BROWNIAN MOVEMENT.

Against the organic nature of the movement in the bion preparations the objection is also typically raised that it is a matter of physical phenomena known as Brownian movement. Brown himself is supposed to have regarded the movement of particles of Indian ink which he observed as signs of life. Physicists explain the phenomenon in a physical manner by the effect of molecular movement. The argument of Brownian movement could only be answered by an experiment to determine whether the particles in Indian ink solution which give the impression of organic movement are culturable or not.

The first experiments in culturing autoclaved Indian ink solution direct on to agar failed. After several mistakes I undertook the following experiment.

Soot was first sterilised dry at 180°C for three hours

and bouillon was stood in test tubes for 24 hours in the thermostat to make certain of its sterility. Several blood-agar and agar culture media were inoculated with soot dry-sterilised at 180° as a control. According to the presupposition, this mixture should give no growth. The result confirmed expectations; dry-sterilised soot gave no growth on agar. Then the soot was heated over a benzine gas flame on a spatula for 2-3 minutes till it was red hot and immediately put into one of the previously mentioned bouillon glasses. In the course of some 10 minutes the dense blackish turbidity became grey, just as with the incandesced coke. After 24 hours in the thermostat the bouillon solution, which now showed a dense yellowish-white turbidity, was inoculated on to egg culture medium. Microscopic examination of the bouillon-soot solution showed bions which were strongly mobile and had the character of coke bions. On the egg culture medium there arose in the course of 24-48 hours a growth of the same colour composed of individual mounds. The bouillon had provided for the infusion of the soot particles, and the egg culture medium supplied the particles with the various nutritional substances. From the top layer of the egg culture medium, blood-agar and agar were now inoculated. Already after 12 hours there developed a soft, creamy, bluish-white growth. At 3000x the microscope showed formations of a different kind to those in the bouillon formations which still showed clearly their origin in the bluish-black soot particles, but were strongly mobile. Soot particles are therefore culturable when all the conditions favourable to life are present. The movement which Brown saw was the movement of soot bions, from which life can develop. I feel fully justified in regarding the experiment here described as the answer to the objection of the physical nature of the movements observed.

The culturing of ash from a central-heating boiler and of wood charcoal was similarly successful. The culture formations had the same character microscopically as the soot cultures.

Of the succession of 18 soot incandescence experiments, only 5 were unsuccessful. The error to which this failure was due could not be established. It subsequently proved that inoculation on an egg culture medium can only produce 1 or 2 round mounds of the size of the head of a small needle, and these must be smeared with the incandesced platinum wire in order to obtain a close growth. Where the growths on egg medium are doubtful I let the culture medium stand at room temperature for 3-14 days. The corrugated growth then appears sooner or later. The test tubes must be kept horizontal.

The result of the incandescence experiments is only astonishing on the assumption that the living formations, which are later seen on infusion, were present before in the form of spores. For then it is incomprehensible how spores could withstand such temperatures. But this view is incorrect. The vesicular elements, which we will call bions, from which the bacteria-like forms develop, have not been present before at all, but only come into existence on incandescence and subsequent infusion through disintegration of the material. The idea arose that all known kinds of spores must once have developed when previously incandescent matter came into contact with water and swelling substances. I finely pulverised flint, incandesced it in the benzine gas flame, and added it to KOH solution. Here also vesicular disintegration and mobile vesicles are to be seen, though only for a short period. Culturing has not so far been succes[illegible]

The view that the living is something entirely divorced from the non-living has hitherto stood in antithesis to the view that "actually everything is living" in the conflict of opinions. Little can be made of such generalisations. Provisionally we must say: It is probable that all unorganic matter contains the capacity for becoming living according to its composition and environment. Life can flicker up everywhere for a shorter or longer time under favourable conditions. Lifelike movement can be seen lasting for a few seconds only. The same movement can be seen on other occasions which only dies after weeks or months. Whether the life that flickers up is maintained, depends upon two fundamental pre-requisites:

1. upon the chemical composition of the substance and,

2. upon the conditions of life, the culture medium, in which the flicker of life is placed.

## Chapter VI.

## CONTROLS AND INSTRUCTIONS FOR CONTROL OF THE BIOL EXPERIMENTS (SUMMARY).

A fundamental pre-requisite for correct control of the experiments and experimental processes described here is not only adherence to all the laws of complete sterilisation, but beyond this the establishment of all the conditions necessary to the organisation of unorganised matter. The following list of conditions is far from exhaustive:

1. The microscope must be capable of a magnification of 3000x with an inclined binocular tube. It is important for the binocular tube to be inclined, since it not only magnifies 50 more than the vertical single tube but also gives a much more plastic image.

2. Hours of observation concentrated on one point in the preparation at a magnification of 2500 to 3000x is essential, besides the slow motion filming of the developmental process. In this way vesicular disintegration of the tissues and crystals as well as the transformation of the forms can be directly observed.

3. An evaluation of the life-promoting effect of boiling at a high temperature or of the incandescing of substances, demands the continual comparison of unboiled and chemically untreated substances with those that have been boiled or incandesced. Only this comparison makes it possible to establish the fact that the mobile formations in the boiled substances are not to be ascribed to the development of spores but to the effect of bursting heat and infusion.

4. The same substance must be observed under conditions of mobility and immobility. This provides the strict contradiction of the movement seen by Brown, as unalterable physical molecular movement; for it can exist under one set of conditions and not under others.

5. The organic character of every movement observed in the microscope can be tested by a culture experiment on the substance concerned. Failure of the culture experiment does not in itself disprove the organic nature of the substance, for there are two possibilities; either, that one is really dealing with a substance that cannot be organised, or that the

substance can be organised but under conditions which are not yet fully known.

6. Control of the findings demands avoidance of an evaluation based on a single finding, e.g. microscopic examination only. A correct finding can only be reached by simultaneous attention to all investigation results, i.e. microscopic, electric, colour reaction, culture experiments, etc.

7. Especially in the culture experiments the following measures are essential.

a) Every experiment in culturing substances which have been sterilised at more than 120° demands a parallel experiment with the same substance in an entirely unsterile state. This enables the difference in the cultures to be clearly established.

b) In the organisation and culture experiments, attention must be paid to the fact that we are not dealing with already-organised formations which are now to propagate themselves in the cultures, but with non-organised matter which has to struggle through to organisation and to this end demands very various and often complex conditions. The organisation of unorganised matter depends principally upon the kind of basic substance and upon the culture medium.

c) The technique of inoculation is extremely important. The first inoculation may give on agar very fine mounds which are hardly visible and cannot yet be identified with certainty as a growth. In bouillon a turbidity may arise of which one cannot say with certainty that it is not a phenomenon of the primary inoculation.

A further inoculation on to another culture medium, e.g. the agar mounds in bouillon or the slightly turbid bouillon on to agar or egg culture medium, may be unsuccessful, but in a series of experiments most of the conditions which make culturing possible can be traced. Organisation must be promoted only after all the laws of sterilisation and the destruction of existing life have been previously carried out.

8. A clear picture of the validity of the experiments is further provided by the serial experiments, both with individual as well as with combined substances. Similarly, unsterile cultures are produced; it can thereby be established

that open agar culture media require days, sometimes even weeks, for the production of a clearly perceptible culture.

The above points, of course, far from exhaust the measures necessary to successful control.

It is naturally important to realise that there are two sources of error in these experiments which must be eliminated. First, the error of insufficient sterilisation, so that formations in the culture or the bion mixture may be regarded as organised organisms which derive from an infecti. But equally dangerous is the other error, of overlooking the conditions necessary to the organisation of lifeless matter, of not establishing them, or not endeavouring to investigate them as closely as the lack of knowledge of their nature demands.

Discussion of the bion experiments without the most exact working out of the conditions would be fruitless and to be avoided. On the other hand, much scientific success is to be expected of controls resting on the basis of these experiments, temporarily ignoring accustomed conceptions during experimental work and thus helping to clarify many open questions.

SUMMARY.

I will now summarise again the most important control experiments which I carried out parallel with the bion experiments of preparation 6.

1. The individual substances of which preparation 6 was composed were sterilised according to their nature and individually inoculated into bouillon. When autoclaved, the individual substances gave no growth.

2. After the addition of every new substance a specimen of th sterile solution was extracted and inoculated in bouillon. The mixtures of substances a+b, a+b+c, a+b+c+d, etc. gave no no growth. The absence of growth in these controls is a certain proof of the sterility of the bion mixture. The tota mixture, autoclaved, must give a growth after 3 to 8 days if all the conditions have been fulfilled. It must be strongly electrically charged.

3. Coke and earth give a high percentage of growth in bouill after being boiled for 1/2 to 1 hour in HCl solution. Coke and earth sterilised dry for an hour at 100°C and inoculated

on agar give no growth. This proves that infusion of the substance is an essential pre-requisite for the development of vegetative life.

4. For comparison of the formations obtained by sterilisation and high temperatures with air germs, agar is left open. The formations obtained are fundamentally different, both macroscopically and microscopically. It takes days, and often weeks, before a strong growth develops.

5. All the substances employed in the production of bions are inoculated entirely unsterile on agar and in bouillon. The agar growths are completely different from the formations from the total mixture after high temperature sterilisation.

6. Moss and grass, after some days in water, are inoculated entirely unsterile on agar. Also unsterile water from the mains. In all cases the growths are distinguishable from the sterile bions of every kind.

SOME PROSPECTS FOR FURTHER WORK.

I have contented myself in these first detailed communications with an account of the experimental findings already confirmed; but in the course of the work there were many phenomena which did not fit in, or were even confusing, and others which opened up new tasks.

If every substance disintegrates into mobile bions under the influence of infusing or disintegrating processes, the assumption suggests itself that the cooked food-stuffs which we consume are subject in digestion to a hitherto unknown process, closely related to the production of bions. Experiments with pepsin on various food-stuffs showed the same phenomena as were obtained by boiling. Pepsin has a disintegrating effect on the substances; their structure is destroyed and replaced, just as in boiling or infusion, by a vesicular structure. Liberated individual formations or heaps of vesicles are mobile. If fresh lymph is observed in the dark field at a sufficiently great magnification the picture is so richly mobile and lively as to give the impression of a bacterial culture. The conjunction of these two observations forces the supposition that nourishment is absorbed into the blood and lymph of the animal in the form of electrically charged vesicular units or heaps. I must emphasise that this is a strongly suggested supposition which cannot yet be either proved or refuted experimentally. The success of such an experiment would be of far-reaching significance, for it would

mean that the body obtains its energy in the form of bions with the aid of digestive processes, just as our org animalcule consumes the individual vesicles. It is conceivable that the cells of the organism then incorporate these vesicles as the bearers of electrical energy. To say or conjecture more at this stage would not be profitable.

Another perspective for further work opens out of the observable effect of highly sterile bions and bion cultures on various kinds of bacteria as well as cells. Whether biological activity of the type of electrical charge is at work here is not yet established. But observable phenomena already permit one to conjecture that a wide variety of experiments can be initiated which will reveal the mechanism at work.

Similarly, there is strong reason to suppose that certa bacteria can enter the organism in two different ways; either through infection, i.e. through a living being invading the body from without and propagating itself and working its destruction there, or else through an inner, still highly obscure, auto-disintegration of the organism, in which protozoal formations develop out of the tissues. The endogenous nature of Mycobacterium tuberculosis (Koch's bacillus) in tuberculosis has long been asserted.

The earth-KCl experiments open up a certain perspective for the understanding of the well-known potassium fertilisat-:ion of the soil. It is possible that potassium fertilisers strongly promote infusion and liberation of electrical energy in the soil so that life energy is freed. It is possible tha the earth bions which are produced have a fructifying effect. This assumption is hard to reject.

I will add briefly that I have succeeded in breeding culturable bion-like formations from human blood. A separate report on this follows.

The methods of bion research proved particularly fruitful for the understanding of cancer in conjunction with sex-clinical experiences in the vegetative function of the human organism. Experiments relating to this were instituted 1 1/2 years ago and show hopeful results.

The work as a whole suffers from a superfluity of problems, which arose all at once, and the inadequacy of the means for carrying it out. I can only hope that there will not be insuperable difficulties in the way of establishing an institute of sufficiently wide range and equipment, together with scientific workers, to deal with the new and manifold problems. In any case, my work suffered not from a lack of problems and solutions, but from the difficulty of setting limits to the sources of these new findings.

Table 1.

Schema of the culturability of earth, culture earth, coke and soot.

| Material | Type of sterilisation | Type of infusion | Electrical reaction | Movement | | Culture in | | Electrical reaction of culture | Ammonia reaction | Remarks |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | | Immediate | 2-3 weeks | 2-6 days | 3-10 weeks | | | |
| Ordinary earth | a | 0 | 0 | $H_2O$ 0 | + | 0 | 0 | 0 | ? | unsteril. mixed cultures |
| " | b | KCl b | ++ | ++ | +++ | ++ | ++ | ++ | ? | |
| Culture earth | b | KCl b | ++ | ++ | +++ | ++ | ? | ++ | ? | |
| Ordinary earth culture | c | KCl c | +++ | +++ | +++ | + | ? | ++ | - | |
| " | d | 0 | 0 | $H_2O$ 0 | + | 0 | 0 | 0 | ? | |
| " | d | KCl c | ++ | +++ | ++ | ++ | ++ | ++ | - | |
| Coke dust | a | 0 | 0 | $H_2O$ 0 | + | 0 | 0 | 0 | ? | unsteril. cultures |
| " | b | KCl b | ++ | + | ++ | + | ? | + | ? | |
| " | c | KCl c | ++ | ++ | ++ | + | + | ++ | - | |
| " | d | 0 | 0 | 0 | 0 | 0 | 0 | 0 | ? | |
| " | d | KCl c | ++ | + | ++ | ++ | ++ | ++ | - | |
| " | e | boull. + KCl | +++ | +++ | ++ | ++ | ? | +++ | - | |
| Soot | a | 0 | + | + | 0 +? | 0 | 0 | 0 | ? | unsteril. mixed cultures |
| " | c | KCl c | +++ | +++ | +++ | ++ | ? | ++ | ? | |
| " | d | 0 | 0 | 0 | 0 | 0 | 0 | 0 | ? | |
| " | e | KCl + boull. | +++ | +++ | +++ | +++ | ? | +++ | - | |

Meaning of the symbols used:

| | |
|---|---|
| a | unsterile. |
| b | boiled at 100°C for ½-1 hour. |
| c | autoclaved at 120°C for ½-1 hour. |
| d | dry-sterilised at 180°C for 1-2 hours. |
| e | incandesced in benzine gas flame for ½-1 minute. |
| bb | twice boiled. |
| cc | twice autoclaved. |
| + | weakly mobile, slightly mobile, occasionally positive. |
| ++ | strongly mobile, frequently positive, clear positive reactic |
| +++ | very lively, preponderant positive result. |
| - | no observable reaction in animals. |

Table 2.

Schema of the electrical charge of various bions and their cultures.

| Type of bion | Electrical charge | Culture | Electrical charge | Remarks |
|---|---|---|---|---|
| Mixture 6 | negative | yes | negative | exception: 6c I = positive |
| Earth | positive | yes | positive | |
| Coke | positive | yes | positive | |
| Soot | positive | yes | positive. Gram-pos. | |
| Muscle-tissue | negative | so far negative | - | |
| Liver | negative | yes | negative | culture findings require further confirmation and interpretation |
| Lung | negative | yes | negative | |
| Egg yolk | negative | no | - | |
| Moss | negative | possibly | negative | |
| Grass | negative | ? | ? | |
| Cow's milk | negative | possible with 120° autoclave | negative | |

APPENDIX

PRODUCTION OF BIONS FROM STERILISED BLOOD CHARCOAL.

1. Blood charcoal powder (cagbo sanguinis) is sterilised in a capsule for 2 hours at 190°C in the dry-steriliser. Bouillon, mixed with an equal part of 0.1 **n.** KCl in two test tubes, is autoclaved for 1/2 hour at 120°. Two other test tubes are filled with pure 0.1 **n.** KCl.

2. A small heap of the sterilised blood charcoal is incandesced in the hemmire gas flame on a metal spatula. Blood-agar is inoculated with this dry as a control. No growth must develop.

3. In a similar manner, another spatula tip-full of blood charcoal is incandesced and tipped into each of the four test tubes. The latter are thoroughly shaken so that the charcoal is equally distributed in the fluid, and placed in the thermostat. After only a short time, a thin grey cloudiness tinges the black colour of the colloid, and after 2h-48 hours becomes a fine, cloudy grey turbidity. It can be demonstrated by gentle shaking.

4. A drop is taken sterile from each of the KCl and bouillon + KCl preparations and examined through an inclined tube at not less than 3000x binocular magnification. The bions must be strongly mobile and exhibit numerous mobile and contractile heaps of vesicles. Electrical testing must show strong positivity (movement to the kathode). If this is the case, fresh egg medium is strongly inoculated and placed horizontally in the thermostat. After 24-48 hours, most of the inoculated egg culture media are more or less closely dotted with small, grey, round heaps. These heaps, if they are sufficiently numerous, are smeared on to the same medium with the incandesced platinum wire and the cultures are replaced in the thermostat.

5. After a further 24 hours, or somewhat longer, a dense, closely-knit, light grey growth with a blue sheen develops. This is transferred to a clear, fresh blood-agar culture medium. Already after 12 to 24 hours, a dense, creamy, blue-grey growth can be seen. Microscopically it consists of a pure culture of round, egg-shaped cocci and numerous strong and swiftly mobile heaps of vesicles, which are contractile. They have a positive electrical charge.

6. These blood charcoal bion cultures are injected into mice

subcutaneously in the back. They should not produce any pathological reaction.

7. Repeated autoclaving of the cultures gives further cultures on egg culture medium.

SUPPLEMENT TO THE PRODUCTION OF BION PREPARATION 6.

The number of successful bion cultures of mixture 6 increased considerably when the method of production described in the first communication of December 1930 was modified as follows (the modifications were partly the result of the findings of M. Roger du Teil's control experiments at Nice and further observation of the production of the blood charcoal bions).

1. Milk is no longer added.

2. Some c.c.'s of bouillon are used in place of Ringer's solution.

3. Incandesced blood charcoal is used instead of charcoal dust or soot.

4. The first inoculation takes place in bouillon + KCl and bouillon alone.

5. The second inoculation is made from the clouded bouillon on fresh egg culture medium. The rounds, which regularly appear, are smeared.

6. The egg growth is inoculated on to blood-agar. The growth is light blue-grey.

## PART TWO.

## THE DIALECTICAL MATERIALIST INTERPRETATION.

### Chapter 1.

### THE PROBLEM OF THE MECHANIC-ELECTRIC TRANSITION.

There is no question of our already being, or even of having any prospect of being, acquainted with all the conditions which lead from mechanical swelling to electrical charge. The second part of the formula of life (mechanical tension - electrical charge - electrical discharge - mechanical relaxation) is more readily comprehended through purely physical principles and through the existence of the first part of the biological function. If there is an electrical charge, this is followed by discharge; if the charge is combined with tumescence, discharge must take place with detumescence. We can already answer one part of the question on the basis of existing scientific knowledge; the other part will be answered sooner or later.

There are substances capable of infusion; where they exist, life can, but need not of necessity, arise.

What maintains organic colloids in continual suspension? Biochemistry has already provided the answer: it is the electrical charge of the particles.

#### 1. CHEMICAL PREREQUISITES OF THE TENSION-CHARGE PROCESS.

Relatively few of the chemical elements are life-specific. According to Hartmann they can be divided into three main groups:

a) carbon
nitrogen
oxygen
hydrogen

b) phosphorus
potassium
calcium
sulphur
iron

magnesium

and also sodium and chlorine in animals.

c) water

We must now assume that each of the above series of substances has a definite relation to the tension-charge process. This implies that the life-function demands precisely these and no other substances for its development.

Let us summarise the life-function once more, from another angle. We shall then see that all life is fundamentally dominated by:

| | | |
|---|---|---|
| charge | / | discharge |
| tumescence | / | detumescence |
| anabolism | / | katabolism |
| tension | / | relaxation |
| assimilation | / | dissimilation |
| growth | / | dying (shrinking, detumescence, discharge) |

Let us now seek the appropriate functions of the individual substances. In this we shall rely entirely upon the existing findings of biochemistry.

living functions,

Among the substances which control ~~carbon~~ and nitrogen are particularly impressive. For they are antithetical in their properties. Carbon has the characteristic of forming complex combinations with itself and with other organic substances, above all of forming proteins. That is, it builds up and binds energy. Nitrogen, on the other hand, has the characteristic of forming very unstable combinations, and consequently, of releasing energy. The assumption is therefore justified that the basic life-functions of tension and relaxation, or the storing-up and discharge or release of energy, have their basis in the above-named antithetical characteristics of carbon and nitrogen.

## 2. ELECTRICAL CHARGE AS A CHARACTERISTIC OF COLLOIDS.

If a parchment membrane is filled with a mixture of starch and sodium chloride solution and hung in a vessel containing distilled water, after a short time salt can be shown to be present in the water of the outer vessel. That is, salt has diffused through the membrane. The starch solution, on the other hand, does not pass through. On the

basis of this difference in relation to membranes, colloid chemistry distinguishes between substances which easily diffuse through animal membranes, as crystalloids, from colloids, which like starch cannot pass through a membrane. Now the colloid is the most essential component of living matter. Colloids are divided into two kinds, suspension colloids and emulsion or lyophilic colloids. Suspension colloids are those which, when evaporated at a constant temperature, are incapable of producing independently a fresh colloidal solution on re-admixture with the dispersion medium, e.g. metallic colloids. Lyophilia colloids, on the contrary, reform a colloidal solution on re-admixture with the solvent after being dehydrated, e.g. proteins.

This characteristic of lyophilic colloids is of decisive significance for the understanding of so-called "germ"-development. If protein can dry-up and then produce afresh an organic colloid on the addition of a certain fluid, it is entirely probable that living unicellular organisms can be dehydrated, that is, become inanimate, and on the re-absorption of fluid become once more a living, functioning colloidal system, i.e. leave the "germ-state" again.

A further characteristic of the colloidal as opposed to the crystalloid solution is that whereas in the crystalloid solution the molecules of the solute are uniformly distributed throughout the solvent, in the colloid the molecules are aggregated in particles and the distribution is uneven. This is demonstrated in the Tyndall phenomenon. If a beam of light is transmitted through a crystalloid solution, its path is invisible when viewed at right angles to the beam. But if the beam is passed through a colloidal sol, the path of the beam is shown by a bright column, due to scattering of the light by the colloidal particles.

Biochemistry also provides a series of explanations of the electrical behaviour of colloidal solutions which are of decisive significance for the understanding of our experiments. If a difference in electrical potential is produced in a colloid, the colloidal particles move, i.e. they are themselves carriers of electrical charge. The nature of the charge depends upon the chemical nature of the colloid and upon the presence of an electrolyte. There is as yet no firm opinion in biochemistry as to the cause of the electrical charge of the finest colloidal particles. It is assumed that the charge of the particles takes place as the result of the attachment of ions of the electrolyte to their surfaces. A colloid which absorbs hydrogen ions must therefore be positively charged. Our experiments suggest, rather, that th

particles themselves, being vesicles, are charged.

Another discovery of biochemistry is very important. The stability of colloidal solutions depends upon the presence of electrical charge of the particles. In the absence of electrical charge of the particles, the solution is no longer stable, i.e. they are no longer held in suspension and sink to the bottom. This is explained by the counteraction of forces. Attractive forces strive to unite the particles, i.e. to precipitate them. The like electric charges, on the other hand, lead to repulsive electric forces. Thus, if the particles are electrically charged they cannot precipitate, the protein cannot coagulate, the solution is stable.

This explanation sounds very satisfactory to me. In the experiments with the bions of boiled preparations, amoeboid mobility and the local vibrating movements of the bions and heaps of vesicles only persist as long as electrical migration is also observable. After some weeks the sediment of the preparation becomes increasingly thick, the solution becomes clearer and microscopic examination shows that both electrical migration and mobility have disappeared.

This forces another conclusion upon us. The tremulous vibrating movement and locomotion of the bions are obviously connected with electrical field effects.

## 3. THE ELECTRICALLY CHARGED VESICLE AS PREREQUISITE.

The structure of inorganic substances such as metals, ores, etc. is "immobile" in the biological sense. Such systems exhibit neither contraction, ~~nor~~ expansion nor locomotion. The hypothetical atomic and molecular "movement" postulates none of the three foregoing basic characteristics of the movement of organised living matter. Attempts have frequently been made to imitate organic movement, e.g. the protoplasmic streaming of Amoeba, by experiments with models. The basis of these model experiments was the conception that organic movement was governed by changes of surface tension. But the surface tension explanation only covers a part of the whole question. At first the development of a spontaneous impulse to movement "from within" remained incomprehensible. Change of surface tension is without doubt a fundamental phenomenon of movement, but it cannot be regarded as its cause. The vitalist would at this point introduce a metaphysical principle to fill in the gap in knowledge. Let us endeavour to approach an understanding of the inner impulse to movement on the basis of the

observations and experiments previously described.

According to present knowledge, the electrical energy in, for instance, iron is bound by a certain organisation of the molecules. By subjecting the piece of iron to an external electrical force, the position of the molecules can be altered, so that the iron becomes magnetic. It is important to observe here that there is a uniform distribution of electrical energy in the atoms and molecules, just as, for instance, in a crystalloid solution. Organised matter differs from unorganised, as stated, primarily in being non-homogeneous and irregular and unevenly dense in structure. The non-uniform nature of the organic colloid is shown by its capacity for scattering light. This difference between organised and unorganised matter must have a fundamental significance for mobility or immobility.

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If the vesicles of various substances are examined in various states, the movements exhibited can be divided into four basic types:

a) The vesicles are immobile, do not change their position in relation to each other (vesicles in gelatine, dust particles, unboiled soot in water, blood charcoal in NaCl, etc.).

b) The vesicles move to and fro locally in an almost rhythmic manner. Locomotion or interaction between the vesicles cannot be observed, e.g. in unboiled milk.

c) Locomotion, e.g. as with Streptococci and Staphylococci. In this kind of movement an interaction of forces between the various vesicles can be seen. If Staphylococci are galvanised long enough to become negatively charged and immobile, they also lose in time the capacity for electrical migration. There must therefore be a definite connection between locomotion and electrical charge.

d) Movement within the organism, i.e. motion of the parts of the organism in relation to each other. It is principally revealed as expansion and contraction.

Fig. 4.

Types of vesicle movement.

a) Motionless.
b) Moving to and fro locally.
c) Locomotion.
d) Amoeboid mobile heaps of vesicles.

The absence of movement in vesicles and the local vibratory movement are problems on their own with which we shall not, for the moment, concern ourselves. Experiment provides the following satisfactory explanation for movement types c) and d). Unboiled and uninfused earth crystals show no kind of internal movement or locomotion in the dark field at even the highest magnifications. On the other hand, vesicularly-disintegrated and infused earth crystals show both internal movement and locomotion. Uninfused, unstructured earth crystals are electrically uncharged; by contrast, the vesicles liberated from earth and coke show a positive electrical charge and strong movement. Even larger units of vesicularly-disintegrated earth or coke crystals mostly show no charge. How is this to be explained?

In the crystal the particles are uninfused and the electrical energy is bound. By infusion or incandescence the mechanical cohesion of the particles in the substance is loosened and the bound energy is freed. Otherwise the vesicl emerging from the crystal could not be electrically charged. The to-and-fro vibrating movement of the vesicles is thus explained by the interaction of the electrical fields of the individual vesicles. If boiled or incandesced earth and coke preparations are observed sufficiently long and closely, two vesicles can frequently be seen vibrating round one another, until a third vesicle approaches; when a certain distance has been reached, one of the two vesicles separates from the other and begins to vibrate round the third approaching vesicle, and so on.

If electrically charged vesicles of any kind are coalesced into "heaps" by lecithin or gelatine, to-and-fro movement of the individual vesicles is regularly replaced by an amoeboid movement of the whole heap, as well as by expansion and contraction of the formation. It can therefore be assumed that locomotion of the whole "vesicle heap" takes place as a result of the interaction of the electrically charged individual vesicles within the mechanically unified heap, e.g. lecithin margin-formation. The vesicles are compressed into a definite area and now continue to vibrate to and fro within the enclosed space; in this way the whole formation attains a movement of its own. There arises within the whole an antithesis or contradiction between the mutually attractive or repellant vesicles in the interior, on the one hand, and the encompassing membrane, i.e. the surface tension, on the other. The electrical charge of the units inside the membrane is thus the cause of both expansion and contraction. If, for instance, there is a preponderance of the repellent forces of individual vesicles at a point on the margin of a vesicle heap, the pressure from within outwards must become

stronger. It may now happen that this internal pressure becomes greater than the resistance offered by the margin. In this case a stretching takes place. The surface tension has decreased in relation to the internal pressure. But this expansion brings about an increase in the surface area which in turn produces a counter-force from the surface. The more the internal pressure has enlarged the surface area, the greater becomes the tendency of the surface to contract again, like a stretched spiral spring. It is not possible at the moment to say what effect this process has on the internal electrical charges. But there can be no doubt of the antithetical effectiveness of surface tension and internal electrical forces as the motivation of movement.

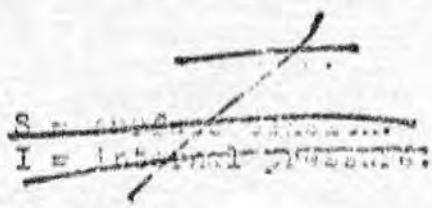
Pauli has proved experimentally that the colloidal protein particles enter into a close relationship with the aqueous dispersion medium. He explains this by saying that during the infusion phenomena water enters the protein. Now, according to Pauli's assumption, the infusion of electrically uncharged protein is always less than that of electrically charged protein. The stronger a protein's capacity for infusion, the more stable is its condition in solution. Proteins which have been little or not at all infused, form very unstable colloid systems, i.e. they precipitate very easily. I should prefer to reverse Pauli's explanation and say that the more a protein can infuse, the easier it can become electrically charged, and the more stable its colloidal solution then is. The reversal of Pauli's explanation has certain advantages. It assists in approaching a solution of the difficult problem of how the infusion of a substance leads to its electrical charging.

The relation of the colloidal sol to the gel is also very important. If dry gelatine is mixed with water, infusion takes place on the absorption of water. The gelatine gel is produced. This finding of biochemistry is confirmed by our experiments, which have shown that if we combine protein substance with water or KCl alone, we obtain a different result than if we add gelatine. In the former case there are no amoeboid formations; in the latter, the colloidal protein has different characteristics, which are closer to those of living functions. On the addition of lecithin and cholesterol, structure-forming forces come into being which, in conjunction with those previously present, bring about amoeboid formations. The gel must therefore be considerably closer to the life-function than the simple sol. The degree to which the dispersion medium is absorbed by the gel is very great. On infusion the volume of the infusable substance increases greatly. Infusion is quite common in lyophilic colloids, e.g.

proteins. Now, if the infusion of a colloid takes place in a confined space, so that increase in volume is prevented, very strong pressure is exerted. It is called infusion pressure, and may reach many atmospheres. In order to express the dispersion medium from an infused colloidal gel, a similar pressure must be exerted upon it.

These facts explain certain important phenomena of our experiments. Those substances which comprise organic life are differentiated in principle from other substances, such as colloidal metals, which also produce colloidal solutions by the fact that, in contrast to the latter, they exhibit infusion pressure.

Let us now proceed a step further in the understanding of the mechano-electric heap, on the basis of biological facts. The first prerequisite of the electrical charging of the particles was thus infusion, an increase in the distance between the particles. The second was that the expansion of the colloidal particle was limited to a certain extent by the formation of a containing margin, a membrane. There then developed an antithesis between the internal pressure, the pressure working from the centre of the infused particle towards the periphery, and the contrary pressure working from the membrane towards the centre. The infusion pressure of a colloid can only appear as internal pressure when opposed by the surface tension of the membrane.



The antithesis of infusion pressure and surface tension becomes eminently important for an understanding of the process of cell division in the formation of the spindle. I shall discuss the relevant facts elsewhere..

## 4. FUNCTIONAL TOTALITY AS THE FUNDAMENTAL CHARACTERISTIC OF LIFE.

It now becomes necessary to tackle the objections that might be raised here by researchers like Hartmann. The amoeboid formations which are produced by the boiling of certain substances, exhibit movements, above all, stretching

and contraction and also locomotion. It might now be said that these are not living organisms at all, but merely surface tension phenomena of a purely physico-chemical nature. Let us listen to Hartmann:

"Living systems, as is generally known, are distinguished from inorganic systems by the possession of highly complex compounds, above all of the large protein molecules. But chemical definitions are fruitless in providing a means of distinction between animate and inanimate molecules, for at the present time no difference between an animate and an inanimate organism can be established from the chemical viewpoint. And even the differentiation between animate or inanimate organic molecules and inorganic molecules, which at first glance seems so easy and certain, proves on closer investigation to be impossible in principle. For the chemist can construct in the laboratory molecules which are entirely similar to the important organic molecules, such as carbohydrates or proteins, present in living cells, but which show no life. A chemical definition and delimitation of the living system is therefore not possible at the present stage of research. Still less can a differentiation be based upon essential physical differences between living and inorganic systems. Living systems are bodies of a colloidal nature, which in this respect show all the characteristics common to inanimate and inorganic colloids. It is precisely the research into the characteristics of colloids during the last 10 to 20 years which has provided astonishing clarification of many apparent peculiarities of living systems. Only if biology, instead of standing as it now does at the beginning of nomothetic research, had already attained the goals of its research, if the whole inter-related physical and chemical processes of the living organism were known, could an adequate physical and chemical definition of life be given."

colloidal

?

This statement of Hartmann's compels a serious consideration of the phenomena of the appearance of living characteristics following the boiling of inanimate substances. The concept of life is far too sharply divided from that of the inorganic; the concept of life involuntarily suggests something entirely disparate from the inanimate. We forget that in the development of the living from the non-living there must be intermediate stages, for which the differentiation of, say, a movement on the basis of the definition "mechanical surface tension" or "living spontaneity" is impossible. But it is out of the question that the laws of surface tension and internal pressure of colloidal substances

are at the basis of living movement in the same way as the para-living movements of, for instance, a prepared oil drop. The point is ~~that life~~ in its completion possesses the quality of development, in contrast to the non-living formations which show movement on the basis of surface tension. A definition of the living as opposed to the non-living on the basis of chemical or physical characteristics can never succeed for physical and chemical functions are common to both domains. Delimitation can only be made, not by denial of the common qualities, but by the discovery of those characteristics of the living which distinguish it from the non-living. According to the perspectives opened up by the bion experiments the difference does not consist in anything which in the living is added and thus makes it living; the difference is a particular combination of functions which are to be found in isolation in the non-living. To repeat, mechanical surface tension and internal pressure appear in the non-living as in the living. The same with the electrical charge of the particles. But the specific combination, the rhythmic alternation of tension, charge, discharge, relaxation, renewed tension, charge, and so on, is what fundamentally characterises life. It is a new function, yet not different in its components from inorganic functions. The combination of inorganic functions into a new kind of function might suffice as a basis for the specifically living primary functions such as, for instance, matter and energy metabolism, division, procreation, etc.

About morphological differences Hartmann says:

> "Here there is, so it seems, such a fundamental and striking difference between the living and inorganic system that one is inclined to assume that living systems can, in principle, be easily and certainly recognised by their morphological characteristics, their shape. A vertebrate, a fish, an insect, a flowering plant, or a fungus, are morphological formations of such a typical and distinctive kind that it naturally seems easy here to distinguish such systems from inorganic ones. But even morphology does not lead directly to the goal, for there are animals like amoebae which are hardly or not at all distinguishable from some colloidal mixtures, and there are plants like small bacteria which cannot be distinguished from the minute granules or drops such as are to be found distributed in the disperse phase of a colloidal system. Thus the morphological definition in general is apparently a failure.

The sole prospect of success is the discovery of the total function, which can indeed be split up into individual

functions of a physical and chemical kind, which are also to be found in the inorganic, but which in the inorganic do not function as a unitary whole. It has hitherto been overlooked that the unitary total function stands in no antithesis to the sum of the individual functions; this provided the vitalists, such as Driesch, with a foundation on which to construct a metaphysical basis for the unexplainable. The task of natural science can only be to ascertain the function which combines the individual physical, chemical and mechanical functions into a totality. We may therefore say that the living differs in no way from the non-living in its individual physico-chemical functions. What gives the living its specific stamp is the combination of the individual functions into a total function of the system, which is dominated by the two basic directions of expansion (towards the world) and contraction (back into the self), as well as by the dialectical alternation of mechanical and electrical. Therefore also, much as I was hitherto in agreement with Hartmann's statements, I regard the following conclusion as incorrect. He writes:

> "If it were possible with present-day means of research fully to uncover and analyse the chemico-energetic processes taking place in the so-called excitation-phenomena, a particular conceptual delimitation of this group of life-processes, summarised in the concept excitation-phenomena, and which often reach over into the psychic side of the higher living systems, would be unnecessary. For we should then be dealing in fact only with more or less insignificant physiological variations of the stationary processes, that is, of the general matter and energy metabolism."

On the basis of what has already been said, the total functions, which characterise life, can never be explained by the most detailed analyses of the individual functions. What is necessary is the unrelenting comprehension of the transition from the sum of the individual functions to the unitary functioning of all these individual functions. Thus a colloid mixture would not be defined as life if it showed the physical and chemical functions characteristic of the living, but only if these functions were combined into a unitary function of the organism in which all the components of every individual function had a place in the total function.

The conception of the organic system as a whole previously stood as metaphysical idealism in opposition to the analysis of the part-functions of the same system as mechanical materialism; they were absolute antitheses,

irreconcilable. With the dialectical resolution of this contradiction between the whole and its component parts, the problem of life is solved in a satisfactory manner by the elimination of the metaphysical other-world-principle as an explanation of the whole.

We need no exalted metaphysical explanation to feel the wonder of life and its functions. It seems to us wonderful enough that we can discern a unitary line from psychic excitation or satisfaction, via the antithesis of the drives of pleasure and anxiety and the physiological antithesis of vagus and sympathetic, the chemical antithesis of potassium and calcium (lecithin and cholesterol), up to the purely mechano-physiological antithesis of internal pressure and surface tension, as well as electrical charge and discharge. The unitary nature of this line from the single inorganic vesicle to the highly complex human psychic functional system is in no way disturbed by the fact that it exhibits such fundamentally different characteristics and complications in the various forms of various functional development.

psychic excitation — psychic satisfaction
(anxiety) (pleasure)
vagus sympathetic
K Ca
lecithin Cholesterol
int. pressure surface tension
+

## Chapter II.

## AN ERROR IN THE DISCUSSION OF "SPONTANEOUS GENERATION".

A full and detailed discussion of the theory of "spontaneous generation", which these experiments render so pressing, is not yet possible. The most important pre-requisites for an adequate survey of the field are lacking. But it is now necessary to point out an error which has crept into earlier discussions of the questions of life-germs or spontaneous generation.

650. In the middle of the 17th century the Dutch natural scientist Leeuwenhoek discovered the Infusoria. He had the idea of soaking all possible substances in water, that is, of preparing "infusions". He took hay, straw, pepper and every possible spice, poured water over them and left them to stand. If he waited long enough the water became filled with remarkable animalcules. It was entirely natural to ask how these minute animalcules came into the water. The first report in Richard Goldschmidt's pamphlet Die Urzeugung ran:

> "The animalcules were so small and seemed of such extraordinary simple construction that it could easily be imagined that they had suddenly developed out of something lifeless. There had previously been nothing living in the substances employed in the infusions, in the pepper or in the hay, all at once they were there, and so there was apparently no other possibility but that the animals which had suddenly appeared had developed out of these substances. Thus the non-living had begotten the living; a concept at which not the least offense was taken."

At about the same time, researches began on the intestinal worms and other parasites in the animal body. Research showed that they developed from the larvae of certain flies. This seemed to strike a heavy blow at the theory of spontaneous generation. The theory of spontaneous generation was replaced by the germ theory, i.e. everything living developed from living germs and not from the non-living. Biogenesis and spontaneous generation thus stood in abrupt antithesis to one another. The English physicist Tyndall's experiment was regarded as a convincing refutation of the theory of spontaneous generation. According to Goldschmidt's report, Tyndall took a number of bottles, filled them with decoctions of 5) different substances and heated them at over 100°C, a temperature beyond which life cannot exist. Being thus certain that they contained nothing living, he found the

bottles and travelled with them to Switzerland. ... the top. six bottles boiled and on examining their contents he found something remarkable. In spite of the boiling, all the decoctions contained a mass of living organisms. The rest ... now divided into two parts. With one lot he went on to a glacier, broke off the necks and left them for several weeks. The other bottles he took into a hayloft of the house and opened them similarly. Some time afterwards he examined all the bottles. Those which had stood in the hayloft, like ... which broke on the way, were filled with all kinds of life, while in those bottles which had stood on the glacier in the pure germ-free, glacier air no trace of life was to be found. "This experiment proves", writes Goldschmidt, "that the development of animals in such decoctions is only possible in impure air".

An obviously effective defence against the thought that the living could develop from the non-living lay in the incorrectness of this argument being overlooked. What did Tyndall do? He decocted substances and after some time found forms of life in hermetically sealed bottles, exactly as happened in my experiments. Some of the bottles he broke in the glacier air, left them open and no forms of life appeared in them. From this he drew the incorrect conclusion that the forms of life he found in the closed bottles had developed from impure air from without. But this conclusion was unjustified. The only correct conclusion from the glacier air-effect might perhaps have been that these forms of life could not maintain themselves in glacier air. The glacier experiment gives no positive explanation of how forms of life can enter decoctions in fused bottles. It is much more probable that the forms of life developed through decoction and were then killed off in the glacier air. At least the question of the development of forms of life from non-living substances should not have been so prematurely answered in the negative. Strict as are the demands of science for counter-checking and confirmation of positive findings, it must be equally strict in the demand for care in the denial of facts. For the fact that forms of life are to be found in decoctions cannot be refuted. The question of "how did the animals enter the fused bottles?" was falsely posed from the start once it is established that forms of life cannot penetrate fused glass.

The rejection of the theory of spontaneous generation seemed to find further support in the epoch-making experiments of Pasteur. He proved that air which is absolutely germ-free never produces anything living when brought into contact with foodstuffs. In Goldschmidt's opinion:

"For the scientific world this, therefore, complete

settled the question of the possibility of a contemporary spontaneous generation, and proved that if forms of life actually appeared anywhere where there previously were none, they had developed from the germs which are everywhere contained in the dust of the air."

This is far from being unambiguously proved, for so-called "germ-free" air does not contain even the finest particles of dust, which in the process of infusion produce forms of life. Pasteur collected the dust of the air and found that there are a multitude of germs capable, if placed in suitable conditions, namely in a damp environment, of re-producing small forms of life. This statement already contains a pre-judgment, for the question of the nature and origin of these germs remains as open as before. Either the germs are forms of life which only sleep, so to speak, and become another form of life, namely bacteria or Protozoa, in which case one would have to know all about the nature of these immobile forms of life called "germs". As far as I am aware, such knowledge does not exist at present. Or else, the germs are very fine non-living particles which under the influence of dampness resulting from infusion are transformed into forms of life, in which case the process of this transformation must be demonstrated by scientific experiment. In no case does the fact that forms of life develop out of dusty air but not out of dust-free air prove that there is no development of the living from the non-living. It might be precisely the fine particles of dust which are the "germs" of the non-living substance from which the forms of life develop. It depends upon the definition of the expression "germ".

There might still be in the dust, bacteria in a dried-up state which come to life on contact with water. Are these dried-up bacteria still forms of life or only "particles of dust"? Thus the germ theory need not necessarily stand in contradiction to the theory of spontaneous generation, once the thought is permitted that the non-living might be transformed into the living as the living is into the non-living. At least the supporters of the germ theory in its present sense owe us an answer as to how the germs as forms of life are created. What characterises them as latent life-forms? What differentiates them from non-living matter? Pasteur's experiments, therefore, do not refute the theory of spontaneous generation, but merely reveal the effect of the dust particles in the air. If decoctions are first examined after 10 or 14 days and life forms are found, the possibility is always conceivable that they have entered it from the air. But if a decoction is brought straight from the steriliser to

the microscope and life-forms are found in this fresh decoction, the theory that they somehow found their way in from outside seems entirely excluded. Otherwise there would be no sterilisation at all. For even ~~[illegible]~~ germs require a certain time to develope. Furthermore, germs should be completely killed off by long boiling at over 100 C. In short, these are contradictions in the germ theory, which need not exist if germs were not regarded as "~~[illegible]~~ COMPLETE life-forms".

At this point an imprecision in the concept of spontaneous generation must be pointed out. By spontaneous generation may be understood that at some time the living developed from the non-living, and then procreated itself as the living. Spontaneous generation may, however, also mean that living life ceaselessly develops out of non-living substances every minute and every second today- If this proof should completely succeed it would, of course, be no contradiction of the development of life from germs, for spontaneously developed life might be maintained by procreation.

Research task

Wide as the perspectives of such a view may be, research must at first be strictly confined to the most exact demonstration of the process of the transformation of the inorganic into the organic. Neither Leeuwenhoek's infusions, nor Tyndall's experiments, nor yet Pasteur's, have anything to say about this process, on account of the fact that no constant observation of the preparation took place. From the non-living dust particle or individual plant fibre till the completely organised microbes or protists become visible, there are innumerable stages of development. There are preliminary stages of organised life, there are stages at which it is difficult to decide whether the particle of earth or the plant fibre is still lifeless or whether it is already alive.

Of some kinds of experimentally-produced life-forms it can be asserted that they are complete life. With others one would hesitate to use this expression and rather assert that they are preliminary or developmental stages of the living substance, such as the crystal which has become plasmoid.

The infusion of a disintegrated plant fibre or an earth crystal is certainly still a mechanical non-living process. But the development of a tube out from an infusing dust particle, for instance, may be thought of as still mechanical or else as already living. Here too it depends entirely upon the conception of what is "living". Hence it seems to me that in the control and reproduction of the facts described it is of prime importance to follow the details of the

transformation of the inorganic, i.e. the process of [illegible] of vesicular disintegration, of vesicle organisation, etc. It must be assumed that there are no boundaries between vegetable and animal, between inorganic, non-living and living. It is necessary that one frees oneself of the static, mechanical way of thinking and endeavours to grasp the process and the dynamic functionally.

SUMMARY OF THE FOREGOING DISCUSSION OF SPONTANEOUS GENERATION.

1. The theory of spontaneous generation has never been finally refuted. Leading natural scientists regarded it as an inevitable consequence of scientific thinking.

2. The aim of the opponents of spontaneous generation has always been to prove that the formations in question are killed by high temperatures. The supporters of the theory were not in a position to provide an exact proof of spontaneous generation.

3. Investigations immediately after decoction were not undertaken.

4. Experiments in culturing the formations produced by decoction were not undertaken.

5. The transitional stages from the non-living to the living were not discovered experimentally.

6. Observations at magnifications of 2000-4000x were not possible.

7. Exact research at temperatures above 180°C was not made, because, beyond the certainty of the destruction of existing life, the possibility of the development of life at temperatures above the destruction-limit was overlooked.

## CHAPTER III.

## THE DIALECTICAL MATERIALIST METHOD OF INVESTIGATION AND THOUGHT.

### 1. OUR BASIC METHODOLOGICAL VIEWPOINT IN EXPERIMENTAL WORK.

It would not have been possible to carry out the bion experiments if the work had not been supported, apart from the basic conceptions of dialectical materialism, by a particular attitude to scientific research work. I shall endeavour to outline this attitude. The scientific worker who is to think over and perhaps check the findings must also have a feeling of the atmosphere in which the work was accomplished.

There is an essential difference between scientific work which sorts out, standardises, elaborates and details that which is already known, which operates in known fields, and research work which must, to begin with, do without all such otherwise beneficent certainties; where, indeed, the uncertainty and questionableness of what is believed to have been observed are fundamental characteristics of the work. Every scientific discovery means an advance into unknown territory; the discoveries mostly stand in more or less strict antithesis to well-known views, the subjective interpretations of ascertained facts. Besides the conquest of the unknown, the second kind of research work has the additional task of coming to grips with habitual trends of thought, refuting them or confirming or elaborating them on another basis.

It is clear that organised scientific work along fixed channels, as for instance in State Hygiene Institutions, employs other methods than those used in unexplored territory. It mostly automatically eliminates everything which contradicts the habitual course of work. It avoids deviating from techniques which have proved themselves reliable and indispensible for particular tasks. It is perhaps an inevitable inconvenience of every scientific advance that although, on the one hand, it discovers the new, in the organisation of the discovery it simultaneously blocks the path of further advance.

Pasteur's discovery of sterilisation was certainly the scientific advance of the last century, which opened the whole of medicine on a new basis. But, as du Bois has already emphasised in his lectures, it blocked the path to the investigation of processes beyond the highest known limits of

sterilisation. Pioneering research work must therefore, of necessity, at first be disliked and stumble upon obstacles. Habitual scientific technique is disturbed. The investigation of new fields demands, let us admit it, a certain disrespect for accepted procedures and explanations.

staining

Since contemporary bacteriological technique has the destruction of life as its fundamental task, it was only natural that the investigation of microbes should employ destruction and the staining of that which had been destroyed as its basic technique. Through staining of the preparations structures were defined and maintained and thereby made available for continuous study. In our work, just on the contrary, it had to be a principle only to work on living, mobile matter, since to begin with it was precisely the mobility and not the static, the function and not the structure, which was important.

coal

If one is sufficiently sure of one's critical self-control, one may dare to make bold leaps into the unknown strictly forbidden in ordinary scientific work. At the commencement of the work, for instance, it was absolutely indispensable that the substances were mixed together unsteri If I had worked from the start with high sterilisation, the difference between the low degree of mobility in the unsteril preparations and the mobile nature of the highly sterile mixtures immediately after preparation would not have been revealed. The infusion, and particularly the culturing, of highly sterile coke particles seemed very unusual to me myself at the first experiments. But precisely this departure into a completely new procedure made possible the clarification of the spore theory. A prerequisite for the correctness of such leaps is, however, that the controls are really also carried out subsequently. But scientific research work frequently suffocates in a superfluity of controls.

There are scientists who will not even look at a findin until they have a painfully exact description of the experimental apparatus before them. Perhaps that is an exaggeration. It is certainly extraordinarily important that one does not, for example, immediately identify a movement under the microscope on current transmission as cataphoresis, but first uses an apparatus which makes possible reversal of current and enables cataphoresis to be distinguished from a fluid movement. But the apparatus must not become more important than the phenomenon one wants to understand.

Another attitude is highly regrettable. A researcher observes a phenomenon, e.g. movement in Indian ink, and

announces it. It is named after him and called Brownian movement. Brown himself believed that he was dealing with a living phenomenon. The physicists explain it as a purely physical phenomenon caused by molecular movement. Against the opinion of its discoverer, Brownian movement is now maintained as a concept by which, from now on, every microscopic movement which does not originate in a well-known manner from the living, will be explained. It is not pleasant to continually hear an expression used when one demonstrates phenomena of a different kind which it does not cover at all. A physicist admitted to me that he taught about Brownian movement in high school, but that he had never yet observed it himself. In Romeis' handbook of bacteriology Brownian movement is not even mentioned; it belongs to the non-living, has been explained as non-living once and for all, so one must not occupy oneself with it further. It had, therefore, to become a fundamental methodological law of my work to take over every technical acquisition of scientific research, to register exactly every practical observation, but, in order not to be disturbed, to begin with to disregard every theoretical exposition. In order to eliminate difficulties from the work at the start, I am in the habit of expressly asking my collaborators only to employ their factual knowledge and technique, but in relation to the work and to myself to forget any theories or views they may have learnt. In this way it was possible to carry out the experiment of incandescing, infusing in bouillon + KCl, and even culturing, soot. It turned out that this so-called physical movement, which as such would have to be present always, on a change of the conditions is not present, that the soot and coke dust lies perfectly still, that movement ceases after several weeks or months. The indolence of thinking in scientific work is a phenomenon of which one becomes very clearly aware on effecting a break-through which sounds improbable. In the second kind of scientific research, therefore, one has not only to overcome facts and problems, and not only to combat and refute views, often false, which have been handed down; but in addition, besides the feeling of the accuracy and correctness of one's assumptions, which one must of necessity have, one has to combat a ceaseless and tormenting doubt in one's own beliefs, which may be very fruitful but which may also lead to the work being prematurely abandoned. I heard from a university lecturer that he had formerly worked in the same direction, but had given the work up on account of the insurmountable difficulties that were put in his path.

So much for the basic attitudes which are essential to work of this kind. Their leading principle is: believe nothing, convince oneself with one's own eyes, and never lose sight of a fact that has once been observed till it has been

thoroughly uncovered.

Just as important is the methodological attitude of dialectical materialist thinking and working, which we consciously employ. For considerations of accuracy the living is usually killed so that life may be studied in that which is dead. The result is bound to be mechanical ideas about life. A dialectical materialist approach, which thus differs fundamentally from the mechanical, necessitates studying life in an organism which is still living. The individual living organism, or even a detail of this organism, cannot be investigated merely as such; the basic principle of the dynami of life governs not only everything which is alive, not only the individual organism as a whole, but as a matter of course every part of the individual organism as well. To think simultaneously of detail and totality, a principle which ceaselessly accompanied and guided the work, is a further pre-requisite for the productivity of scientific research. Mechanical conceptions of living processes are necessarily methodologically erroneous. Hope is pinned to the prospect that a more detailed understanding of the synthetic movements of organic substances might one day reveal the essential natur of life. But the essential factor in life is not the complex matter, but the complex function. Concepts such as biogen, micelle, energid, etc. are merely aids in the work. They do not correspond to any facts. They are attempts to substitute the effect of a "substance" for the understanding of a functio "They" express something regarding the mechano-chemical proces only, but become metaphysical when used to explain the functio Since they are merely auxiliary concepts and contain no facts or functions they frequently hinder research more than they further it. It merely introduces a fresh unknown, as long as the function itself is not materially comprehensible and reproducable. The poverty of the chemico-mechanical conceptio of life consists in the fact that it endeavours to reach the whole by the addition of details, instead of searching for the function of the whole in every individual part. For our basic methodological approach there is no difference between the plasma streaming of an Amoeba, which can be seen, and the vegetative current that can be felt in certain states of excitation. The function of a tree is not explained by a demonstration of the chemical composition of cellulose. The leaf branches out from the twig in precisely the same way as the twigs from the branches, the branches from the main stem, and the leaf veins from the main vein. A unity governs the whole.

Physiology, for example, describes the paths of impulses

and believes that in so doing it has explained the impulse itself. The way to the understanding of the total biological function of the whole is blocked by the mechanical explanation of phenomena studied in their physico-chemical aspects, and by the division of the whole organism into separate parts. It also, for example, splits states of mind and moods from vegetative functions; for instance: "Joy reddens the countenance" (an idealistic metaphysical conception of the process), or "Sweating follows anxiety", or "There is no organ dependent upon the vegetative nervous system which is not influenced in its function by this or that mood".

The function of the organism is thought of in sociological terms. The brain is the "highest centre", the "governor of the organism" corresponding to, say, the ruler of a state. But the brain is one of the later developments of the organism. There are organisms with no brain at all, so the brain cannot be the centre of function. The vegetative system is more primitive and older than the brain and hence the origin and centre in a functional sense. In the brain it has created for itself a switchboard. Life without a brain is possible, a brain without vegetative life is impossible. The source of such errors of thought is the intellectualistic projection of one's own idea of function on to reality. For decades the conception ruled that vegetatively innervated organs do not communicate feeling. In opposition to this, however, it is to-day measurably established that ego-perception is the reflection of the vegetative excitations active at the moment. Contraction of the sympathetic is directly felt when one stands on the edge of an abyss. One feels pleasure as expansion, stretching, widening. It is a necessary assumption, following from the identity of organ perception and vegetative life, that even the worm must feel itself, for vegetative current is identical with the perception of pleasure and unpleasure.

These remarks from another field were necessary, for they too characterise a part of the basic approach to the work. It is true that measurement and experimental repetition have the last word in scientific work; but when I watch an Amoeba stretch and the protoplasm in it streaming, I react to this observation with my whole organism. The identity of my vegetative body-perception with the objectively visible plasma streaming of the Amoeba is directly evident to me. I feel it undeniably. It would be mistaken to derive a scientific theory from it yet, but to draw conviction and the strength for strict scientific work from vegetative, involuntary acts of perception of this kind is an indispensible prerequisite of productive research.

## 2. THE DIALECTICAL MATERIALIST LAW OF DEVELOPMENT.

### (a) The conflict between mechanism and vitalism in biology.

In contrast to mechanical materialism of the Buchnerian type, as well as to the no longer absolute but dialectical idealism of Hegel, the materialist dialectic represents an infinitely fruitful, though hitherto relatively little utilised, enrichment of natural scientific thought. It is naturally not sufficient simply to boast the materialist dialectic as the better method or to discuss it in abstract philosophical debates, to pursue method per se. It is necessary to test the method of dialectical materialism concretely on actual problems of natural science and society, to apply it to them afresh, to prove its superiority, to show that in fact more can be comprehended and attained with it then with mechanistic methods.

Let us briefly summarise the bases of the dialectical materialist method.

Mechanical materialism asserts that development takes place in a "causal series"; that is, every phenomenon has its cause, but this cause is itself the result of an earlier cause, and so on. But it is not in a position to make really comprehensible the manner in which process B "arises" from process A. Hence it has to assume a "principle of development" which, however, first needs to be explained itself. Why is there development at all? This question it can only cope with by calling upon a "force" inherent in matter, which is nothing else than the metaphysician's "spirit". But where do the force or the spirit come from, which have this effect? Dialectical materialism, on the contrary, asserts that development is explained by the presence within matter of antitheses which produce a contradiction. Within a particular situation the contradiction cannot be resolved, so the antitheses press towards a change of the situation, and thus something new arises. This new state arose through resolution of the contradiction, but itself develops new contradictions which press towards a new resolution, and so on. Only thus does it become comprehensible that everything happens in a eternal flux, that everything at some point arises and likewise passes away again. There is thus nothing isolated and absolute, everything stands in correlation to other events.

To the mechanical view the antitheses are absolute, mutually incompatible. Dialectical materialism sees the

identity of opposites; they may proceed from one another. Hate is not only the opposite of love, but can proceed from love. Much conscious love is unconscious hate and, *vice versa*, conscious hate is often unconscious love.

The mechanical view does not only see things as eternal, it also sees development. It was precisely mechanistic science which from the Age of Reason carried through the idea of development. But with regard to its world-view it represents the standpoint that development is gradual only, that there are no sudden changes. The materialist dialectic sees that gradual development is followed by sudden changes, that evolution prepares for the leap in development. Now, scientific research very often refutes mechanistic philosophy just there where it correctly sees sudden changes in the developmental process.

Mechanistic as well as idealistic philosophy denies the possibility that quantity can proceed from quality and *vice versa*. Dialectical materialism, on the contrary, asserts that quantity not only can change into quality, and *vice versa*, but that this change is amongst the bases of every natural process.

Scientific knowledge goes forward much more slowly than the urge for knowledge; from endeavours to construct an immediate philosophical interpretation of natural processes as a whole on the basis of the results of research arose natural philosophical theories. Among these two main tendencies can be distinguished - mechanical materialism and vitalism. The researcher does not only reach certain general views on the basis of his own and other findings, he mostly approaches things, consciously or unconsciously, with a fundamental general philosophical viewpoint; theory formation also depends upon the researcher's philosophical presuppositions. The problem of the psycho-physical frontier cannot be approached without a knowledge of the basic scientific-critical theories. I will therefore summarise briefly the difference between the mechanistic materialist and vitalist positions.

The materialist theoretician tries to comprehend the organism as a machine; he fathoms the energy processes, the chemistry, the physiological laws, and asserts that the same laws are valid in the organic as in the inorganic, but that their bases are more complex and therefore harder to grasp. But, in principle, knowledge of the physical, chemical and physiological processes are sufficient for an explanation of the living.

The vitalist, whose critical position has for the moment considerable advantages in consequence of the lack of knowledge, raises an objection. In the organic there are, in addition to the causal relationships of the parts, a "purpose" and the effect of the whole, to which the parts are subordinate. Development is not wilful, but follows definite forms which cannot be explained causally. A goal, telos, relates the development of the individual parts to the whole, to the species, etc. This goal is directed towards preservation; it aims at preservation of the whole, formation of the whole and propagation of the species.

The mechanist retorts that this applies also to crystal and chemical combinations, only here the relation of the parts to the whole is easier to analyse; here too no part can be removed without destroying the whole. Thus there is no difference in principle from the organic.

To this the vitalist objects that the sum of the parts still does not give a functioning organism; the whole is governed by an aim, an idea. This, we know, is the same objection raised by the spiritual against the natural sciences, by the theoreticians of values against the materialists.

The mechanist is able to point to Köhler's "physical configurations", which prove the wholeness of inorganic systems.

But the vitalist knows how to maintain his position; he says: hydrogen and oxygen can be chemically demonstrated, also their combination into water. But $H_2O$ shows completely new characteristics, which were present neither in H nor in O, they are therefore not comprehensible, but "irrational", and will never be comprehensible mechanistically. The qualities ("green", "red", "fluid", etc.) are specifically new characteristics and hence inaccessible to reason; similarly with the problem of form and species. The physics of the consciousness-process analyses vibrations, but gives no explanation of the colour "green".

physics //

psi //

A mechanist, such as Max Hartmann, replies to this that natural science has no interest in the explanation of the irrational. The irrational remnant of existence lies outside the sphere of science; the purpose explains nothing, for it requires explanation itself. The purpose-principle merely provides starting points for research into the constitution of matter, in otherwise uncharted territories.

The vitalist, such as Driesch, volunteers to prove the necessity of the purpose-principle. Every cell is a part, but at the same time it can develop the whole. This speaks in favour of the "prospective potentiality" of the organic. One can never produce a whole machine from a screw. The living organism, on the contrary, is a "harmonious equipotential system" (Driesch).

The mechanist replies that every cell contains the whole chemistry which makes differentiation possible. What is needed is knowledge of the colloidal fine structure. It is true that there is a gap here, but that is no proof of the impossibility of the explanation.

The vitalist points to heredity and egg division. Every egg can produce the whole; from the standpoint of mechanism, therefore, one would have to assume a three-dimensional machine in the egg. But division from one original egg cell would exclude the machine, for a machine cannot be divided so many times and still remain whole.

The mechanist points to chromosome division, i.e. to the equal division of potentialities.

The vitalist replies that auto-division cannot be explained mechanistically.

Enough, the conflict runs thus into infinity and will remain undecided. I mentioned its bases in order to clarify our basic methodological approach, which differs fundamentally from both the mechanist materialist and the vitalist.

(b) The three dialectical systems.

Dialectical materialism denies the existence of a purpose-principle in nature, that is, the existence of a supernatural goal beyond the energetic process, which is supposed, according to the general view, to determine development. But it denies equally the presence of a mechanistic causal principle. If we assume one material force, which is supposed to govern the development of any process, we have at the same moment unconsciously re-introduced a metaphysical principle existing and working beyond matter and energy. Natural science as a whole is to-day dominated by the two above-named principles of thought and explanation - metaphysic fatalism and mechanical causalism. Their refutation sets dialectical materialism the difficult task of proving that it is capable of resolving contradictions and providing explanations which remain closed to the other methods of thought. It

must prove itself practically by its capacity to solve experimentally, in a particular way, problems which were hitherto insoluble. Naturally, in the last resort, its own basic principle of proving theory by practice and forming of both a unity applies to dialectical materialism as a work method.

The exponent of mechanical development and the philosopher of purpose will say: "Alright, all development proceeds from antitheses and contradictions. We understand that when force A comes into conflict with force B, a third thing, something new, must result, process C, in which, however, A and B are still present. C comes up against D, and so the process goes on. But what about organic development? There is germ development without the germ coming into contact with an outer force. It is clear that development here results from a principle within the germ. But where did this inner contradiction, which now impels development from within outwards, arise? This inner contradiction must also have arisen at some point!"

This critical question is entirely justified. It is not enough for dialectical materialism to assert that a new, third process results from two opposed forces. It is indispensible to make clear the nature of the inner contradiction, and to comprehend its function. If we survey the scientific arsenal of dialectical materialism we must admit that this question has not, hitherto, been either posed or answered. It was difficult, till now, to find a place for biological development in dialectical materialist thought.

It is necessary to distinguish three kinds of antitheses in nature:

a) Antithesis of systems (system antithesis). If physical system A meets physical system B, say in the movement of two bullets, both must somehow change direction (or magnitude). Similarly, the relationship between the centripetal and centifugal forces, and the result, the revolution of the planets, is a typical example of a system antithesis.

b) Dissociative antithesis. If a neutral piece of iron approaches the positive end of a magnet, iron and magnet enter into a system antithesis to one another. This has a certain effect on the iron, which is expressed in an inner re-organisation of the molecules. The neutral iron becomes negative on the side turned towards the magnet and positive on the side turned away from it. In the unitary piece of

iron two opposing poles have arisen in consequence of the effect on it of the magnet. If the magnet is removed, the previous antithesis is resolved again by the disappearance of the magnetic poles, the iron again becomes neutral. The "environment" (the magnet) makes the iron itself magnetic, i.e. a part of the environment is now "inside the iron". In other terminology we should say that an outer contradiction or antithesis became an inner contradiction. The process of dissociative antithesis is illustrated in the following diagram:

Fig. 5.

Dissociative antithesis (as in magnetism, electricity, acid-base-salt, etc.)

Positive and negative electricity is also subject to the law of dissociative antithesis; as well as chemical dissociation, as in the reaction:

$$NaOH + HCl = NaCl + H_2O$$

In the process of dissociation there arises from the neutral state the tension of antitheses which presses towards resolution of the contradiction, i.e. towards relaxation. Tension and relaxation are, therefore, not sequential, independent processes, but, in the process of tension arises also the motive for relaxation, just as relaxation contains the essential elements of fresh tensio

If the antithetical chemical compounds sodium hydroxide an sulphuric acid react and combine, they produce the unitary neutral product sodium sulphate and water.

Let us look at this mutual relationship of tension and relaxation in the simile of a steel spring. If the spring is wound up, the "impulse" to return to the state of relaxation appears in the spring to the same degree as the tension produced by the winding up. If the spring is released it automatically returns to the relaxed condition Organic life differs from the mechanically tensed inanimat spring by the fact that it can tense itself again by itsel

c) Genetic antithesis. So far we have dealt only with the alternation of unification and dissociation. In a living cell, diasociation - by division of the nucleus - takes place internally before division of the cell. The fusion of two cells which have been thus internally dissociated

means a re-unification of opposites. But the process of cell division differs from simple dissociation in that it proceeds in a geometric progression. A cell divides to form two, each of these divides again to form four, these again to form eight, sixteen, and so on, as in the following diagram.

Fig. 6.

Genetic dissociation.

We shall return later to the problem of how the genetically-divided can be subsequently re-unified. We can say in advance that in the growth of a biological organism, cell division is accompanied by a continual integration of the new cells into a unitary function of the total organism. If we now survey the three foregoing basic forms of dialectical process in nature we shall see that:

1. System antithesis is universally valid. There is no spher of existence in which one body does not stand in some relatio to another, whether in the interaction of molecules or in the relationships between men or between planets.

2. Dissociative antithesis, in the form of mechanics, chemistry or electricity, governs inorganic as well as organi existence.

3. Genetic antithesis is peculiar to organic existence. Antithesis between two bodies in space may be the cause of dissociative antithesis (magnetic iron), just as dissociative antithesis may produce genetic dissociation (cell division). All three types of antithesis are found in the processes of organic life. System antithesis and dissociation antithesis are common to both organic and inorganic existence. The organic differs from the inorganic by the function of genetic dissociation and antithesis. Copulation and conception represent the synthesis of two antitheses in a new third entity. Procreation is a function of dissociation, in that procreation of the individual can only take place as a result of dissociation of the parent body. In Protozoa, procreation takes place by nuclear division (dissociation) and cell fission (division). In a sexual process, the fusion of two germ cells (synthesis of the dissociative antitheses) can onl take place after gametogenesis (dissociation) in the parent organisms.

Let us consider the fundamental processes of chemical dissociation and electro-magnetism. Every dissociation or

division of a unity into antitheses means at the same time a liberation of energy or, which is the same thing, the establishment of a tension which presses for solution. Every synthesis of antitheses re-binds free energy, i.e. produces relaxation. In the crystal, sodium chloride is electrically neutral, due to the stable arrangement of atoms and molecules. On solution in water, ionic dissociation into sodium kations and chlorine anions produces respectively positively and negatively charged particles which are antithetical. The stable equilibrium of the crystal is replaced by a dynamic equilibrium - tension - in the solution. Attempts to increase the tension, by increasing either the solubility product or the ionic product, of the solution are resisted. If the solution is concentrated beyond the saturation point, the tension between the particles is resolved by re-formation of neutral crystals; the antithesis between the charged ions is synthesised by their unification in the stable and electrically neutral unitary crystal. Similarly, in a piece of non-magnetic iron the effects of the molecular magnets counter-act and cancel each other. The piece of iron as a whole is magnetically unified and exhibits no external magnetic tension. If, under the influence of a magnet, the internal arrangement of the molecules is destroyed, the piece of iron itself becomes a magnet. An antithetical tension is produced between its north and south poles, and an external tension - magnetic field - is produced. We see this in the fact that the previously neutral piece of iron is now capable of performing work. It is able, for instance, to bring another piece of iron out of its state of rest, which it previously could not. Similarly, a dissociated electrolyte can now perform work by means of the ion-current, but not the neutral salt.

Let us now apply these relationships to the basic processes of the biological functions. First we see that sperm and ovum are energetically highly tense antithetical systems, which press towards the resolution of tension in the fusion. After the synthesis, the previous external antithesis of each is replaced by an internal one; an energetic process of division commences in the fertilised ovum. The system antithesis between ovum and sperm leads directly to the function of genetic dissociation of development in geometric progression. One ovum produces 2, then 4, 8, 16 cells.

It is self-evident that everything living must exhibit a common basic rhythm, a common basic law, which it must be possible to observe in every detail of living existence. Division of the life process into biological, psychic, ideological, cultural, etc. fields is, it must not be forgotten, an artificial procedure springing from practical

convenience. In what manner does the inner antithesis in a biological system, such as the fertilised ovum, arise and lead to genetic differentiation, to biological development? Let us begin by taking our bearings from a region well-known to us from character-clinical work, as to how this internal-isation produces an external antithesis and what form it takes with regard to development.

A child who, at the age of 2 or 3, harmlessly plays with its excreta has, in the absence of complications, a unitary affirmative structure in respect of this impulse. The impulse to play with excreta now comes up against socially imposed educational demands of cleanliness. We are faced with a basic type of system antithesis formation. This antithesis now leads to a conflict between the child and the educator. Under the persistent influence of prohibiting education, the external antithesis is transformed into an internal one. As the first result of this transformation we see fear of the educator appear in the child, contradicting the impulse to play with excreta. This fear is already the first step in the formation of an inner psychic conflict. The psychic structure of the child exhibits a progressive cleavage into two parts. The instinctual demand to play with excreta remains in existence, anxiety acts as a preventative. Something in the child, which we call the ego, begins to defend itself against the impulse and develops a moral principle out of fear of punishment. Previously it ran: "I'm afraid to play with excreta", it now runs: "I dont want to play with excreta, it's dirty". The external antithesis became an inner antithesis. This was followed by a dissociation, a splitting up of the unitary impulse. This internalised conflict became the source of a "development" on the basis of genetic antithesis. The antithesis between wanting-to-smear-excreta and defending-onself-against-it leads the child to develop a predilection for painting or drawing; out of the inner conflict arose something new, not present before and composed of both antithetical determinants, representing a synthesis of the antithesis. For when the child now paints or draws, it unites all the demands, both instinctual and moral; for drawing is "clean", in contrast to the smearing of excreta. Painting or drawing are culturally and socially important and recognised achievements. So we see how the old system antithesis, as well as the original inner conflict antithesis between impulse and morality, have been united in the new sublimation product "painting". The following diagram will illustrate this process. We will pass over the interesting question of how such a synthesis splits up again. What must be borne in mind is that the process of dissociation and opposition shown in the diagram - and as a dynamic, not a static process - governs all existence that is subject to

development.

## Fig. 7.

### The process of dissociation and opposition.

Every developmental process contains the following functions united into one common function:

a) system antithesis (the whole of existence)
b) dissociative antithesis or division (through internalisation of the system antithesis)
c) opposition of the divided forces
d) re-unification of this division with subsequent division and synthesis in geometric progression (only in the organic).

Let us test the foregoing in some fundamental processes. Organic life proceeds from the inorganic. It contains the specific functions of the inorganic - mechanics, electricity, chemistry; but in the process of becoming it develops its own laws, on the basis of which it stands in contrast to the inorganic as a special realm. Thus, for instance, the plant to the nutrient fluid of the matrix; the org animalcule to the bion of the same plant, in that it absorbs, consumes, this bion; or man, who is a part of nature, to nature itself.

The new organic life is now divided further, after having entered into an antithetical relationship with the inorganic (above all in the act of eating), in that consciousness is now split off from vegetative life. But vegetative life and conscious life, sprung from one root, likewise enter into an antithetical relationship (as previously vegetative life with the inorganic) in the form of self-perception. Life perceives itself, in that the function of consciousness makes an object of its own origin, i.e. the vegetative life. In the realm of sociology the process of dissociation and opposition is shown in, for instance, religion and sexuality. They were originally identical, for nature religion was nothing else than a collective orgastic experience. With the emergence of private economy and sex morality, orgastic experience in society was divided. Sex-affirming religion became a sex-negating supernatural doctrine which from now on opposed ~~raged against~~ its origin; religion stands *contra* sexuality. Religion has become the negation of sexuality - its antithesis - in spite of a common origin. In the new ideology of sex-affirmation a further step in this development takes place, in that sexuality makes contact with religion and is unified with it in recognition of vegetative life.

In scientific development also, we see that a new science proceeds from an old one through division, in consequence of inner contradictions and resultant opposition. The old science of psychiatry, because of its non-recognitio of psychic life, forced psychoanalysis to develop as a separate branch. As a science psychoanalysis opposed the ol psychiatry. Analytical psychology now contained in itself a contradiction between the clinical doctrine of repression an the culture theory. This resulted - to begin with as an attempt at solution within the system of psychoanalysis - in the theory of the unity of instinct and culture, in the form of the scientific orgasm theory. There now arose an interna contradiction (orgasm theory and psychoanalytical culture theory) which led to a cleavage of the unitary psychoanalysi into the death instinct theory on the one hand and sex-economic orgasm theory on the other. In the further course of development, sex-economy opposed psychoanalysis as an antithesis, at the same time representing a new formation, which solved many contradictions of its predecessor, e.g. those between sexuality and work, instinct and morality, nature and culture, etc.

After this digression, let us return to a matter close to the theme. One of the most striking examples is the antithesis of sexuality and anxiety, or parasympathetic and sympathetic. In origin they are identical, a unity, belongi functionally to the same vegetative system, but the unitary vegetative function splits into two antithetical directions and functions of flow, which now oppose each other. Anxiety becomes the negation of sexual excitation, it corresponds to a direction of electrical energy function and current flow antithetical to sexuality. Parasympathetic and sympathetic become antitheses. The following diagram illustrates the foregoing.

**Fig. 8.**

**The unity and antithesis of parasympathetic (pleasure) and sympathetic (anxiety) in the vegetative nervous system.**

We do not reflect dialectically upon life. The proces of nature is dialectical *per se*. The division of the unitar and the opposition of antitheses, far from being mere acts o thought, can be observed, measured and photographed in the process of reality. Every living being is, by virtue of its vegetative apparatus, a part of living nature as a whole, th result of the division of one vegetative life into milliards

of kinds of life. And every living being stands in antithetical relation to both the rest of the vegetative world and to each single vegetative individual. Be it in the act of eating, be it in the sex act, be it finally as the passive object of eating. The following digram illustrates the process of vegetative division and opposition.

Fig. 9.

Vegetative division and opposition in organic life.

Since Darwin there can be no doubt about the splitting up of the genus into species. The dialectical law of development (division and antithesis, plus synthesis) is the basic law of every organic development. The branching out of the tree trunk into branches, then twigs, these into petioles and these petioles into leaves, the leaves into a central vein, from which again fine branch veins of the first, second, third, etc. degree proceed, is an example of the dialectical process in the reality of nature.

In the following illustration (58) we see division and opposition depicted in the leaf-arrangement of a plant.

The essential concept for the understanding of the dialectical function of life is that this division does not disturb the unitary nature of the whole. The vegetative nervous system, divided and antithetical as it is, nevertheless functions as a unitary indivisible whole.

Fig. 10.

Diagrammatic representation of the unity of living functions.

Let us now endeavour to bring this basic law closer to the actual subject of the present work. It is a question of how the multicellular function is related to the unicellular function. The metazoon - such as the human body - was thought of as being composed of many single cells, wherein certain groups of cells had certain individual functions, such as gall secretion, urine secretion, blood corpuscle formation, innervation, etc. The body was thought of as constructed on the pattern of a well-functioning state. "At the apex" stand the brain and endrocrine secretions, which

direct the functions, and various "tasks" were delegated to the various organs. True, "eating" and "procreation" were thought of as functions of the whole organism, but as though there were something in the organism which "wants to preserve individuals and the species" - an entirely metaphysical principle. The same functions were also attributed to the protozoon. But we know that the organism of the metazoon as an entirety functions unitarily just as that of the protozoon. The metazoon stretches out, just like Amoeba, as a whole organism. It contracts into itself in fright as a whole, just as Amoeba draws in all its pseudopodia. A fundamental law therefore pervades all living existence irrespective of all differentiations and antitheses.

It was at first surprising, even startling, to find that the antitheses of psychic pleasure and anxiety prove to be functionally identical with the physiological parasympathetic and sympathetic function; that parasympathetic and sympathetic function are absolutely identical with the organic substances lecithin and cholesterol, and these in turn absolutely identical with the effect of inorganic substances such as potassium and calcium. If we reverse the series from potassium or calcium, via lecithin or cholesterol, to parasympathetic or sympathetic function, and finally to pleasure or anxiety, we see the evolution of one function to higher, more complex, more differentiated forms. Therein the basic character of life entirely governs the whole development and all its products. This is the antithesis between the functions of expansion and contraction, tension and relaxation, charge and discharge.

## 3. SOME REMARKS ON BIOGENESIS.

Now that we have become acquainted with the general laws of dialectical materialist and functional thinking, we may take the step of testing its special application to the development of life. These are only provisional orientation

Inorganic and organic are not two strictly separate, unrelated domains, not linked by any transition. Since organic life exhibits in principle the same physical and chemical processes - above all mechanics and electricity - as the inorganic, inorganic and organic are functionally identical in their basic characteristics. Thus far our methodological approach coincides with that of the materialist mechanist. But we differ from the latter by the fact that, besides this functional identity, and simultaneously

with it, we have to assume a functional antithesis. It is not a matter of assuming besides a functional identity a functional antithesis in theoretical abstraction, but of finding practical proof of the existence of the latter and revealing its structure concretely. Hartmann writes in the section "Concept and Range of General Biology" in his major work Allgemeine Biologie (3rd Edit.):

"It is not yet possible to determine and define life, as is the case with certain natural inorganic bodies e.g. minerals and crystals, by a definite chemical composition and a definite material structure; for with living bodies there are, beyond this, processes of a quite particular kind which stamp the bodies and systems in question as living, and in the absence or cessation of which one can no longer speak of life.

"Chemical and mineralogical bodies etc. are systems maintained in a stable equilibrium, a genuine chemical equilibrium. True, with living systems we are also dealing with bodies which create the impression of a more or less permanent physical system. But what a difference! There is no question here of stable equilibria, the individual permanence is in a physico-chemical respect only apparent. It is only attained by a continuous metabolism, a continual building up and breaking down of the component chemical substances and a continuous alteration of energetic processes; it is a matter of ~~anabolic and catabolic~~ dynamic equilibria only. A continuous stream of anabolic-katabolic processes flows through living systems and maintains their apparent equilibrium."

Hartmann emphasises that ~~matter and energy~~ metabolism, excitation phenomena and change of shape are the criteria under which the life processes can be summarised - which are shown to be characteristic of all life.

We are now faced with the question of the origin of these basic characteristics of life themselves, particularly excitation phenomena and change of shape; for if they are assumed from the start, so to speak, as an explanatory principle, the question of their nature and mechanics in comparison with inorganic life remains open.

The functional identity of inorganic and organic is comprised in mechanics and electricity. Our experimental findings have shown:

1. The basic factors of mechanics and electricity which

govern the inorganic, combine in the living in a particular alternation of function which is precisely its determining factor. There is no inorganic process in which mechanical filling (swelling) becomes electrical charge, then electrical discharge and mechanical relaxation. The foregoing series of mechanical and electrical functions is specific to the living and distinguishes it from the inorganic.

2. The tension - charge - discharge - relaxation process not only distinguishes the living from the non-living, but must, if the previous statement is correct, fundamentally govern al living functioning. It must, therefore, be possible to demonstrate that all vegetative functions follow the above-named rhythm of tension, charge, discharge, relaxation. This formula, which we may call the life formula, was discovered i the function of the orgasm. The formula might just as well, though not so easily, have been observed in the automatic movement of the intestine, of the heart, in cell division, et

3. The life formula (or orgasm formula) governs every vegetative function. Not only in the whole biological organism, but also in each of its parts; the individual cell of the metazoon is subject to the law of life which we summarise as the tension-charge formula, both in its own realm and as a part of the total organism. It must, there-:fore, be possible to demonstrate the tension-charge function in the unitary total function of any living organism whatever as in the separate function of individual cells. It remains a prerequisite for this that the mechanical and the chemo-electric processes are not investigated and observed separate-:ly, but always in their relation to the life rhythm.

Life in toto, as a basically unitary function, proceeds from the inorganic. But, within the living, voluntary activity developed out of vegetative, unconscious functioning Similarly, the highest developmental product of its time - consciousness with all its functions. We are naturally far from even beginning to understand the differences within vegetative life. We already survey a part of the common basis, but we do not yet know all those circumstances and conditions which lead to the development of a new function from an old one within the process of life.

**Fig. 11.**

**Diagrammatic representation of the unity of living and non-living functions.**

| | | |
|---|---|---|
| Inorganic | potassium ..... | swelling |
| | calcium ..... | shrinking |
| | charge - discharge | |
| Organic | lecithin ..... | swelling |
| | cholesterol ..... | shrinking |
| Living | parasympathetic ... | swelling --> charge |
| | sympathetic ..... | discharge -> shrinking |

So much for the fundamentals. The following assumptions are developed on the basis of the foregoing viewpoints:

1. Life cannot have arisen through "spontaneous generation" in the sense that at some time, at one particular spot in the universe, life somehow came into being and then ceaselessly propagated itself. The theory of cosmic generation, i.e. the migration of living spores from other parts of the universe to the earth, is unproven and also improbable. It would be necessary, at least, to demonstrate it as probable that the conditions for such a transference of life exist, and that protein filaments or spores could have survived all the obstacles and extreme conditions of this journey.

2. Direct observation of living processes, particularly the vegetative, force us to the conclusion that life arose under entirely simple and natural conditions out of inorganic matter.

3. These natural and simple conditions exist to-day and everywhere. Life requires certain substances with certain characteristics, primarily carbon, oxygen, hydrogen and nitrogen; these substances are everywhere! To bring them to living functioning requires certain conditions, which make possible the leap from mechanical filling (swelling) to electrical charge. These conditions, both of swelling and of the electrical process, are present everywhere.

4. Life can be generated at any time and in any place where the necessary substances and conditions are to be found. Nature possesses neither balances nor electrical apparatus. There must, therefore, be a principle which establishes the

choice of the quality and quantity of the necessary conditions. This is the principle of self-regulation in the generation of life. Life thus arises hourly everywhere. It only remains to comprehend it in its entirety.

5. Everything that furthers swelling and charge, furthers life.

6. It is necessary to differentiate between original, continuously-arising life and life which is the propagation of original or already-propagated life. In original life there is a further distinction between that which arises from disintegrated organisms and that which springs from the organisation of the inorganic.

7. When living matter arises out of non-living and becomes again non-living, there is a circulation between inorganic and living, as well as within the living. The multicellular organism disintegrates into single cells and the inorganic when it dies. From both sources the single cell is re-composed.

8. In principle, death is to be understood as a cessation of one of the three main functions of the rhythm of life:

a) Shrinking or lack of the swelling substance, i.e. principally water, already renders the first phase of biological functioning - mechanical tension - impossible (death of thirst).

b) There is a disturbance in the transition from swelling to charge. It cuts off the automatic life function (death from a heart attack).

c) There may be a disturbance in the transition from discharge to shrinking.

d) Finally, the transition from shrinking to renewed swelling may be deranged.

Observation of the death of etherised experimental mice shows phases in the process of dying. To begin with the organism attempts to defend itself against the poison by increased motor activity. The mouse collapses, breathing quickly. Breathing becomes irregular and finally ceases. But death has not yet intervened. On the cessation of breathing the whole body begins to twitch. In this twitching the body's electrical energy is discharged. Renewed charge does not occur, since breathing and even combustion no longer take pla

The twitching ebbs away, but sometimes the heart beats a little longer. Thus one function after the other is extinguished. Restitution of life would be possible or not according to their relative importance to the life function.

Some day the means will be found to re-establish the functions, above all of swelling and charge. But such hopes belong to scientific rêverie. We will not deny ourselves them. Many a dream of to-day becomes the reality of to-morrow!

# LIFE AND MATTER

by

Prof. ROGER DU TEIL

(translation)

NOTE: The following lecture by Prof. Roger du Teil, here translated under the title "Life and Matter", forms the first part of his contribution to the joint volume, Die Bione, under the title "Leben und Materie", p. 117-125.

While the second part of his section in Die Bione, "Drei Versuchsreihen auf Grund des 'Spannung-Ladung'-Prinzips" (p. 126-135), is included in the Biotechnic Press translation of Die Bione (under the title "Three Series of Experiments on the basis of the 'Tension-Charge' Principle"), the part which follows has been kept separate as it was not considered suitable for inclusion in the translation.

# LIFE AND MATTER.

by

PROF. ROGER DU TEIL.

Delivered at a meeting of the Académie des Sciences Morales, Nice. 18 September 1937.

The following exposition is divided into two parts. Only the second comes within the true province of the Académie des Sciences Morales, although the first is nevertheless, a necessary preliminary to the second. It is concerned with the philosophical interpretation of certain very recent biological experiments, which are, to some extent even, still in progress. It is therefore necessary for me to summarise briefly the course and results of this biological work in order to draw from it the conclusions which are the actual subject of these discussions.

Dr. Wilhelm Reich, a former co-worker of Freud's, who is already well-known for the remarkable results of his investigations into the relations between electricity and vegetative life, has for some time been engaged in Oslo on experiments in the development of unicellular organisms, possessing all the characteristics of life, out of absolutely sterile chemical substances. In his opinion, they "derive their life" from the electrical charges liberated in a colloidal solution. In January of this year he informed me of the positive results of these investigations. He had succeeded in culturing several generations of organisms exhibiting all the characteristics of life - mobility, alternation of expansion and contraction, growth, and even division. He invited me to control these findings, which had in addition, mostly been filmed. To this end I have been engaged for some months in a French laboratory on experiments parallel with those of Dr. Reich, and have exchanged with him both my observations and sample cultures of these "bions" - as he has called these micro-organisms.

This is not the place to detail the strictly scientific nature of these investigations and the measures taken to ensure absolute sterilisation, by temperatures of 120°C for fluids and 160° for solid substances. Their mixture was sterilised afresh at 180°, and immediately thereafter these organisms appeared. Since some scientists, in spite of the immediacy of their appearance, raised the hypothetical

objection that the bions might have developed from spores contained in one of the materials used, which had escaped sterilisation, Dr. Reich and myself pointed out that even the existence of germs resistant to a temperature of 130° for more than two hours was an impressive discovery at the present stage of sterilisation methods. Since then, Dr. Reich has produced further proofs. He expanded the field of his work and showed that finely pulverised coke, and likewise silica crystals, after being sterilised at 160° in KCl solution, disintegrate into innumerable spherical vesicles, from which - as the microscope and the camera, which filmed the process, show - first spore-like and then amoeboid forms of unicellular organisms develop after a few days. They possess all the characteristics of life and are culturable. While boiling at 130° destroys all previously known living germs, it constitutes by contrast - and this is the main point of the experiment - an essential prerequisite to the appearance of the germs under discussion.

Dr. Reich's work, whose findings have in part already been confirmed by me and which are, at my request, at present being controlled in several laboratories in France and Belgium, has been described in detail to the various learned societies concerned with purely scientific matters.

Since, however, certain opposition views have proved to have a metaphysical basis, it seemed to me to be necessary to sketch more closely here the scientific and philosophical range of our work.

In its simplest form, Dr. Reich's main idea, since the commencement of his investigations into the electrical charge of the erogenous zones of the human body and the tension changes produced in them by the experience of certain feelings (such as pleasure or pain), consisted in the possibility of equating the vegetative life processes with an electro-mechanical process which he named "tension-charge". On the other hand, Dr. Reich disclaims having, at the present stage of these biological investigations, created life by a kind of miracle, and emphasises that, according to his formula, "life everywhere proceeds from the non-living" spontaneously. He assures us that he has merely discovered experimentally the process by which life sprang from matter, and is perhaps still springing. That is, by the liberation of intra-molecular electrical energy from matter in certain states, such as the colloid, and at certain high temperatures.

It seemed to me at first that this interpretation, which would make of life a particular state of matter, could be countered by a, so to speak, antithetical viewpoint. Accordi

to this, everything would be living, and what we now call "matter" would in reality be a form of life which could be described as "virtual". This "virtual life" would be capable of becoming "actual" - of proceeding, under certain conditions and under the influence of certain factors, from latency to manifestation. In short, that which has hitherto been known as "life" would be only a narrow section of real life. This word would have described only the form of "actual" life, i.e. the form adapted to the contemporary conditions of our planet. Since this adaptation is essentially expressed in nutrition, i.e. in the organisation of chemical substances in a liquid dissolved state, and since these substances coagulate or evaporate at high temperatures, "once-adapted" life in its "actual" form can no longer resist the temperatures which made possible its appearance, or rather, its transition from virtual to actual.

Pasteur's discovery of sterilisation thus prevented researchers for a century from seeking the origin of actual life above certain temperatures, which were known to destroy it in so far as it was organised. Consequently, the origin of actual life out of virtual was continually overlooked.

But even if this interpretation has the synthetic character of every monism, on closer consideration it is only nominally satisfactory; for it is just as materialistic as the previous one. The conditions for the transition from virtual to actual, as well as the factors which bring this transition about, change of temperature and electrical energy belong in fact to the realm of matter.

Various materialistically inclined scientists have recognised the value of both these experiments and this interpretation without the least difficulty. I myself - as I shall describe in greater detail in conclusion - am inclined to regard them as a boundary which human understanding, with its desire for unity, naturally strives for as a modern and scientifically tested interpretation of an extended Spinozism. But the same cannot be said of the idealists, some of whom have informed us of their views, and still less of the dualists, who represent the most dogmatic attitude of modern idealism. It was with them in mind that I wished to express to-day my - at least provisional - opinion. I am convinced that their fear springs from a definition of life which, it seems to me, precisely in view of these experiments, must to-day be transcended, without, however, abandoning the framework of dualism.

For the present, we will pass by the fact that the dividing line between life and non-life is tending to become

more and more blurred, to a point where a practical differentiation between the two realms becomes impossible. The observations of Cartaud, Dauzère, Mary, Otto von Schroen, on the pseudo-cellular bodies of colloidal solutions, and those of Harting, Rainey, Dastre, and those of Raphael Dubois on the "eobes", and Martin Kuckuck's on barium cytodes, meet here; as well as those of Herrera on protobia, and the remarkable discoveries concerning chemical viruses which Jean Rostand has recently published in France. They show that all attempts at drawing a dividing line between single celled organisms, which reproduce themselves by simple division, and physico-chemical formations in electrical equilibrium or disequilibrium, are becoming more and more illusory. This is entirely confirmed by Dr. Reich's work, for he has incontrovertably succeeded in uncovering and even reproducing the process by which unicellular life proceeds from chemical matter, and he has been able, by culturing them, to place these products finally in a domain which has hitherto been regarded as specifically vital.

It is absolutely necessary to escape from this schism if one does not wish to exhaust oneself in empty controversy between an apparently triumphant materialism and an idealism which feels its foundations in danger. This idealism lands in a hardly scientific but understandable defence reaction that goes so far as to deny the reality and validity of these experiments, in spite of the support provided by our innumeral controls.

The attitude that we have adopted - at least provisionally - is, in my opinion, the only one which offers a way out of these difficulties. We admit the validity of the experiments, that is, the possibility of reproducing by an electro-chemical procedure the processes by which nature seems to make living out of non-living. I say "seems" - for I hesitate to acknowledge that what is thus produced is really life. More clearly, these phenomena, which coincide so exactly with those of life, are "life" in the accepted sense of the word. But my assertion is that it is this sense that must be changed. And I do not hesitate, in conformity with the new conception of life which I propose, to exclude not only the organisms produced by various researchers but also everything known to-day as unicellular life and included among the elementary organisms. In other words, I propose that the boundary between living and non-living should be drawn, not between organised and non-organised substances, but at another point which I shall proceed to indicate.

This conception is not so bold as it may seem at first glance. Years ago biologists divided animal cells into two groups, having different, in fact antithetical, functions in relation to a cardinal point, namely the continuance of life. They differentiated between the germ plasm, the transmitter of life, and the soma, whose function is clearly only to bear the germ, to nourish it and to enable it to await the moment when it can renew the duration of life. There thus appears to exist a difference in nature between germ plasm and soma, in spite of their necessary mutual interaction and connection. We know, for example, that characteristics developed by the soma only are not inheritable, in contrast to what develops from the germ plasm. The distinction between life and non-life which we now propose to adopt is no other than that already effected between germ plasm and soma. It is merely an extension, a general interpretation, of this former distinction.

Matter may, in fact, be induced to organise itself into complete physico-chemical formations by electrical charges liberated when it enters a colloidal state. These formations may, by osmosis, absorb certain chemical substances in which they are immersed and thus develop a function which resembles metabolism. Electro-chemical disequilibrium may cause them, after attaining a certain size, to divide and so introduce a reproductive process. These various phenomena may seem so improbable that their laws must remain unknown to us, and their unpredictability, like that of the sub-atomic world, may give the appearance of freedom - an appearance which has already upset the theory of tropisms. But none of this compels us to regard the peculiarities of these formations as not attributable to the realm of matter, and consequently to assign them to that of life. Furthermore, in view of the correspondence of all characteristics, we have to admit that there is no means of distinguishing what we previously regarded as life and as irreducible, from what we observe to have been developed artificially by material processes and must logically ascribe the life - artificial or genuine, this distinction has no further meaning here - produced in our experiments to the realm of matter.

But on the other hand, none of these electro-physico-chemical processes will ever explain how, from the fusion of two cells, or even, in the frequent cases of parthenogenesis, from one cell which divides not homogeneously but heterogeneously, a number of cells can develop which are different from one another in both form and function, and how finally an organism arises corresponding to one and always the same type, which we call the species. In any case, it will not be possible to explain in this way the development under such varied environmental conditions of an identical type of such great complexity. Prediction breaks down here. One has to

assume, as Bergson pointed out, if not a purpose in the narrow sense of a preconceived plan, previously attached to this word, at least an organising intention of life. And consequently, if we want to preserve for the concept "life" a specific and irreducible meaning - as indeed we must - we have to assign to life its place in this organising intention, which is "idea". We shall therefore systematically ascribe all phenomena and all, even the organised, components which comprise the soma, to the so-called realm of matter. Formerly the substratum of life was regarded as essentially the chemical substances of which the cells are built up. But its form, the transition from latency to manifestation, was looked upon as the work of the spirit. In future we shall say that the substratum of life consists of all organised cells of the soma insofar as they reproduce themselves homogeneously by simple division. The proper domain of life we shall reserve for the organisation, or rather "the organising intention of the somatic unity, insofar as the soma in its entirety is subordinate to the germ plasm, and the dynamism of the germ plasm itself, insofar as it is the creator of the soma, which is destined to bear the next germ". And by thus preserving for life something which is solely "idea" we believe that we have deflected from idealism the danger which might have developed out of a too far-reaching interpretation, or an interpretation based upon a too far-reaching definition of life of a discovery which viwed from this angle, on the contrary, lends idealism new forces.

But this is not all. By narrowing the definition of life, we simultaneously broaden that of matter and, so to speak, spiritualise it. In future we shall no longer be faced as in the original, narrow form of idealism, with spirit and matter having no common source and no mutual interaction, in which one cannot trace coincidence to necessity, freedom to universal predetermination, and consciousness to phenomena, which was the source of so many insoluble problems. We shall have a matter exhibiting so many common characteristics with spirit that mutual interaction is no longer inexplicable.

Furthermore, the results of such an interpretation are not confined to the field of biology. We can now apply them to psychology. Let us assign to matter the whole substratum of psychic life, i.e. organic and even affective life, but let us reserve for life proper, idea formation, the complex forms of intelligence, will and consciousness, everything which is intrinsically spiritual activity! Certainly we are aware that in man the lowest, most undifferentiated, manifestitations of vegetative and affective life are stamped by the spirit, just as the highest forms of spirituality have corporeal roots. But here too our suggested distinction may prove fruitful. It saves us from the further necessity of

searching for an explanation from spirit or conscious will of phenomena of an entirely material automatism, in which a process is directly followed by the most probable reaction. We shall be able to grant materialism the validity of its views in many points. Bergson saw this very clearly when he drew the limit of freedom, not between living and non-living (in the contemporary sense), but between surface automatism and unpredictable choice of the deep ego - i.e. between soma and germ plasm - if it is true that the germ plasm, by virtue of the fact that it is transmitted, comprises our real being. And does not the distinction between two forms of memory made by Bergson correspond to the same striving for a delimitation of the two natures which are divided in and comprise the human being? We shall, therefore, ascribe motor memory, as usual, to matter, and shall thus have no difficulty in understanding how spiritual memory, which nevertheless accompanies the former, transcends the human body.

It will be objected that this is a matter of terminology Undoubtedly - for we are endeavouring to show that in practice this distinction is already complete. We only wish to gain it recognition. Terminology can often explain obscure questions and prepare the way for the establishment of a law. The grouping of a body of previously unconnected phenomena under the name of electricity made possible their mastery.

Psychology leads us quite logically to metaphysics, which has so often proved closely related to the former. William James was already fully aware, when he suggested the reversal of what might be called the genetic order of phenomena - i.e. that the emotions should be regarded merely as the perception of the peripheral movements of our bodies - that such a conception would have nothing materialistic about it, but, on the contrary, rescued idealism by insisting upon its meaning as the essential factor in emotion. The analogous re-arrangement which we are advocating to-day, merely generalises James's. Whether one proceeds from pluralism, or returns to the distinction formerly so sharply drawn by Christianity between body and soul, or whether, like modern psychology, one reaches an immanentistic monism, the highest idealism will always remain untouched. It seems, furthermore, as though this last group of experiments tends to tip the balance towards a doctrine of immanence. No change in our highest spiritual viewpoints would be made necessary by the discovery that spirit, from its first attempts onwards, endowed ordinary matter with new capacities and characteristic and to living matter adds its own nature, its whole more subtle intention, the whole purposefulness already virtually manifested in the organisation itself - an intuition which,

on the highest rung of the ladder of existence, develops into reasoning thought. We have been able to follow this development through the course of centuries, from the original protoplasm of the ocean up to man, and we have now perhaps succeeded in reproducing experimentally its first step. But one sees it repeated in every moment of our lives when the interaction, the purely biochemical inter-play of our cells gives birth to our psychic lives. With the possibility of explaining experimentally, by simple electrical forces, most of the phenomena of our vegetative, that is our affective, life, a psychology supported by these new discoveries will become capable of shaking off the yoke of the old, so-called spiritualistic conceptions - which are in truth animistic - which hindered it and barred its access to a whole field which is, in fact, open to experiment. By admitting that the spirit is capable of transcending, as intention and meaning, the pseudo-vital and pseudo-psychic phenomena, thanks to which it is transformed and becomes rational, we believe that these phenomena open the wide field, disclosed by William James and fructified by Bergson, to that practical knowledge which alone enables the spirit to save itself.

# THREE SERIES OF EXPERIMENTS

# ON THE BASIS OF THE "TENSION-CHARGE" PRINCIPLE

by

ROGER DU TEIL.

## THREE SERIES OF EXPERIMENTS ON THE BASIS OF THE "TENSION-CHARGE" PRINCIPLE.

The first two series of experiments here reported accord in principle, though they deviate in application, with the procedures carried out by Dr. Wilhelm Reich of Oslo in conformity with his total biological viewpoint.

The third series, designed by myself, follows the same general line, but deviates considerably from the others in method and results as well as in the conclusions to which it leads. It contains simultaneously a confirmation, a simplification, and a concentration of Dr. Reich's procedure.

Dr. Reich's synthetic theory consists essentially in the equation of the psychic tendencies "Towards the world" and "Into the self, away from the world" and the accompanying feelings of pleasure and anxiety, on the one hand with the parasympathetic and sympathetic nervous systems, which govern the movements of fluids in the organism which accompany these feelings; and on the other hand, with the proteins and other chemical substances which assist these movements in the higher organisms, and in lower organisms replace the nervous systems of metazoa. This equation extends to the electrical phenomena of charge and discharge, which accompany the fluid movements and, together with the alternation of expansion and contraction, constitute life itself in the continually maintained antithesis between "centre" and "periphery".

It is the antithetical pairs lecithin-cholesterol on the one hand, and potassium-calcium on the other, added to a colloidal fluid in which movement develops and increases through the Brownian movement of fine coke particles and in which innumerable marginal membranes form, which evoke in this fluid the formation of organisms showing all the characteristics of life and which are culturable.

The three series of experiments described below were carried out by myself after my return from Oslo. Dr. Reich, whose work, at his request, I had followed and controlled for several years, demonstrated to me in his laboratory in Oslo his own control procedures, which I tested and with which I compared mine. I constructed the apparatus described here to simplify the controls by making them easier and simpler to protect against infection.

FIRST SERIES. THE "BION" EXPERIMENT.

First Experiment.

The apparatus consists essentially of two vessels, one above the other, separated by a glass tap. A tube connects the upper parts of both, so that air can move from the lower to the upper vessel when the fluid in the upper vessel runs into the lower. To the lower vessel is attached an inverted U-shaped capillary outflow tube, to which a test tube containing bouillon to be inoculated is made fast with a rubber stopper. The whole system is hermetically sealed.

Fig. 12.
Closed system.

The original mixtures which comprise the final preparation can thus be placed in the autoclave separated, and with them the bouillon which is to demonstrate the living nature of the organisms. On removal from the autoclave, the two solutions are mixed together by opening the tap. When the set time has passed it is sufficient to heat the upper vessel so that the expanding air forces a drop of the mixture to be tested into the tube of bouillon. In this way, the whole process after removal from the autoclave takes place in a hermetically sealed system.

This apparatus, which I called the closed system, thus eliminates every possibility of a chance infection by an air germ or by a germ introduced during the experiment. In addition, preparation 6, as originally prepared by Reich, had to be replaced in the autoclave after the final mixing in order to destroy any germs which might have been introduced during the experiment. There was always the danger that this sterilisation might destroy the bions as well, or at least weaken their vitality and damage their culture. Since the apparatus makes exogenous infection impossible after the sterilisation which precedes mixture, subsequent sterilisation is omitted and the bions can be cultured just as they arise.

It should be remarked here that in cases where the use of a hermetically sealed apparatus is not demanded by the necessity of establishing the purely endogenous origin of the bions, their presence in the final mixture can be ascertained by microscopic examination of a sample taken immediately after

producing this mixture. By this means, the hypothesis of the development of spores contained in one of the substances employed is excluded.

Further, if this examination, which is made on a cavity slide, lasts any length of time, the warmth of the microscope lamp evokes such a multiplication of the organisms that, in the course of a quarter of an hour or so, the mixture becomes so dense that it ceases to be fit for observation.

1937? On the 3rd September the two vessels were filled as follows. In the lower vessel were placed equal parts of 0.1 % KCl solution and Ringer's solution, some milligrams of red gelatine in 3 c.c. KCl solution, a few cholesterol crystals, 5 mg. of finely pulverised coke which had been heated to redness on a spatula in a bunsen flame, some drops of white of egg, some drops of yolk of egg, and some drops of milk, all sterile. The tap was then closed. In the upper vessel was placed about 10 mg. of lecithin dissolved in 1 c.c. KCl solution.

The tube attached to the lower vessel is filled with sterile bouillon. After the upper stopper has been replaced by sterile cotton wool, which permits the percolation of air, the whole apparatus is placed in the autoclave.

The first sterilisation took place on the 4th September, 3/4 hour at 130°C. The second on the 5th September, 24 hours later, 1 hour at 130°. The third on the 6th September, 24 hours after the second, 1/2 hour at 130°.

After the sterilising, the cotton wool was immediately replaced by the sterilised, precision ground stopper, which was sealed with ~~cotton wool~~ paraffin. The tap was then opened and the solution in the upper vessel mixed with that of the lower. The tap was closed and sealed with paraffin, also the stopper and all joints of the side bouillon tube.

On the 10th September, i.e. 4 days later, the upper vessel was heated and some drops of the fluid inoculated in the bouillon tube. On account of its size the apparatus could not be placed in the incubator. 24 hours later the bouillon had not clouded. Some further drops of the mixture were added by the same method. 24 hours later, on the 12th, some of the now definitely clouded bouillon was inoculated on agar and in bouillon.

From the 13th onwards this bouillon showed a very definite cloudiness, and the agar a grey creamy culture, which

at first consisted of small round colonies and quickly spread over the whole surface.

Both these cultures of the 12th were again inoculated in bouillon and on agar on the 13th. In these four tubes cultures of the same kind developed. Since then, inoculation and culture have proceeded normally.

Microscopic examination of both the first mixture and the cultures showed a very large number of round or oval mobil organisms which, on multiplying, formed numerous clusters and chains. These are bions, as Dr. Reich called them when he first produced them in Oslo.

My investigations have shown that bions are Gram-positi I was further able to establish that they are sensitive to certain poisons. They are killed, that is to say, deprived o mobility, by potassium hypochlorite solution in about 1/4 hou and by alcohol in about 1 minute. Ether dissolves them in a f minutes.

Second experiment.

A similar experiment was made on the 15th September. The same apparatus and the same procedure were employed, with the exception that no milk was added. Bions developed as in the first experiment, but this time they developed earlier, on the 18th September. Once again the whole procedure took place in a closed system, and the bouillon clouded immediatel Microscopic examination showed the bion culture to be much livelier than the original bions, which were less numerous th time than in the first experiment.

Third experiment.

For this a new apparatus was employed. It is construct ed according to the same principles as the first, but is simpler. It consists of 4 connected tubes, 3 in a parallel a one in an inclined plane. The tubes are so constructed that mixture of the two bion solutions and inoculation in bouillon can be effected by simply tilting the apparatus. There is al a control tube containing bouillon which is subjected to the same processes but is not inoculated. I called it the closed tube system. (See Fig. 2.)

On the 18th September bions were produced in this appar atus. Milk was not added to the mixture. Since colourless

gelatine was used, a trace of methylene blue was added, whereupon the mixture took on a colour indicative of its gelatine content. The whole was autoclaved for an hour at 130°. Mixture was effected by tilting the apparatus. The mixture assumed a greenish, opalescent colour, reminiscent of absinthe. Inoculation was carried out 2 days later, i.e. on the 20th, and on the 21st a very definite clouding appeared, similar to the previous ones. On the 22nd some drops of the culture were forced through the capillary tube by heating the apparatus, and inoculated on agar and gelatine-protein culture media. Cultures developed from both inoculations.

Fig. 13.

Closed tube system.

A. Mixture 1.
B. Mixture 2.
C. Bouillon to be inoculated.
D. Control bouillon.

(Footnote: Instructions for the closed tube system.
The lecithin mixture goes in tube A, the KCl-Ringer's-coke etc. in tube B (small quantities only to be used). The bouillon now goes into tube D. The stopper in D is now pressed into the tube so that it closes the connection with C, so that a premature inoculation cannot take place during autoclaving or manipulation.
The whole is then autoclaved.
Thereafter the contents of A and B are mixed together in either A or B, preferably in A.
For inoculation, a drop of the completed mixture is drawn into the previously empty tube C, the stopper in D is slightly withdrawn so as to free the connecting tube and permit the bouillon to run over from D to C. A little bouillon remains in D as a control.
A wad of cotton wool can now be inserted in D to prevent infection of the control bouillon by the air of the other tubes.)

Microscopic examination of the bouillon inoculated in the closed system, revealed a number of chains of oval bions, but also rigid lecithin tubes within which gocci were forming

The presence of these lecithin tubes was explained by the fact that the inoculation was rather heavy and these forms were introduced together with the mixture of which they were a component.

Microscopic preparations of this particularly characteristic bouillon culture were stained with carbol fuchsin and laid down for preservation.

SECOND SERIES. THE ~~SO-CALLED~~ "INCALDESCENT COKE" EXPERIMENT.

First experiment.

On the 19th August the following mixture was prepared in a test tube: about 10 c.c. KCl solution, a drop of liquid sterile gelatinated blood, about 0.1 g. finely pulverised incandesced coke dust. (The remainder of the gelatinated blood is still perfectly sterile today, 23rd September.)

On the 28th August bouillon was inoculated from this mixture, with a positive result - regular turbidity.

On the 29th August an inoculation was made from the bouillon on to gelatine, with a positive result - a grey culture similar to those of the bions.

On the same day, 29th August, the original bouillon tube was boiled twice for 1/2 hour at 100° in the open air, then bouillon was inoculated from it, with a positive result.

On the 3rd September the original tube was autoclaved for 1/2 hour at 134°. Inoculation in bouillon produced a positive result. On the 4th inoculation was made from this bouillon on to gelatine, with a positive result.

On the 5th September the original tube was again autoclaved at 130° for 3/4 hour. Inoculation was made from it in bouillon, with a positive result. To avoid all chance of infection the inoculation was made as follows. The sterile fluid was put in a sterile dropper, which itself acted as a stopper to the tube containing the bouillon to be inoculated. The actual inoculation took place automatically through the increase of pressure in the autoclave, therefore, under conditions which entirely excluded any contact with the air, and at a temperature recognised as fatal to every germ.

On the 6th gelatine was inoculated from this bouillon, with a positive result.

Second and third experiments.

On the 27th and 28th September the same experiment was repeated twice, but with sterile bouillon substituted for gelatinated blood, and a variation of the proportions of bouillon and KCl from one occasion to the other.

Both these experiments, like the first, and in spite of every form of sterilisation of the original tube, produced positive results.

THIRD SERIES. THE ~~SO-CALLED~~ "EXPERIMENT WITH PURE KCl".

Of the substances used in the foregoing series of experiments, the sterility of the red- and even white-heated coke and of the bouillon could not be doubted, on the basis of the Pasteurian hypothesis. There remained the possibility of suspecting the KCl, although this was obtained pure and was dissolved by 1 1/2 hours' continuous boiling at 100°.

This third series of experiments was undertaken with the aim of clarifying the rôle of the KCl in the previous experiments.

On the 31st August a phial of this ~~three times~~ THRICE boiled solution was placed in the autoclave and sterilised in the usual way. Bouillon was immediately inoculated from it, with a positive result. An inoculation from this bouillon on agar produced a very strong culture.

On the 3rd September the same phial of KCl (a drop-counting phial which makes automatic inoculation possible) was again autoclaved for 1/4 hour at 130°. Inoculation in bouill gave a positive result.

On the 5th September the same phial was again placed in the autoclave. At the same time KCl from this phial was placed in a sterilised dropper resting in the tube of bouillo Inoculation was effected by the steam pressure inside the autoclave, i.e. under conditions excluding air and at a temperature of 130°, with a positive result. Inoculation of this bouillon on to agar on the 8th September produced a very fine grey culture, which took longer to develop than the previous ones.

On the 8th September the KCl was once more autoclaved in the same phial and inoculated direct on to agar. A positi result appeared after 4 days in the shape of a light grey,

almost white culture. Re-inoculation on to another gelatine medium on the 11th September again produced a positive result - a very fine whitish-grey culture.

At this point I had two h-tubes made. These consist essentially of two test tubes, one of which has had its mouth fused into the side of the other at about the middle, its lower half being parallel with the straight tube. The single opening, situated in the upper part, can either be closed with a rubber stopper or fused over a flame, in either case, therefore, hermetically sealed. A fine capillary tube is fused into the side of the straight tube and makes possible subsequent withdrawal of the untouched fluid. (See Fig. 3)

On the 8th September KCl from the phial, which had now been sterilised 5 times, was placed in the straight part of the apparatus, and sterile bouillon in the bent part. The whole was autoclaved for an hour at 130°. When it was taken out of the autoclave the bouillon was poured on to the small quantity of KCl at the lower end of the other tube by simply tilting the apparatus. The bouillon was thereby inoculated. Some bouillon was left in the bent tube as a control.

**Fig. 14.**

**h-tube.**

A. 1st phase, KCl.
   2nd phase, KCl + bouillon.
B. 1st phase, bouillon.
   2nd phase, control bouillon.

This experiment, which was carried out under conditions of absolute sterility with regard to both the substances used and the apparatus, produced a positive result. From the second day the bouillon in the straight tube, compared with t control, showed a very weak but perceptible turbidity. A re-inoculation on gelatine carried out on the 14th produced afte some days a culture which was at first very weak and colourle After a few more days, however, a grey culture developed wher the drops of bouillon had fallen. This culture seemed to rea favourably to a lower temperature than that of the incubator. Two microscopic samples taken from the weak and the creamy parts of the culture showed the complete similarity of the organisms that had developed. In the one that appeared later the tendency to chain formation was more marked.

On the 9th September a similar experiment was carried out with the second h-tube, also with a positive result. While the clouding of the original bouillon is scarcely noticeable, a re-inoculation on gelatine made 11 days later very quickly produces a transparent culture, entirely similar to the first, but very strong. On microscopic examination it shows the same organisms.

It should be added, that several experiments in which KCl was heated to redness and dropped into bouillon, also led to markedly positive results. By contrast, an experiment with KCl which was fused during incandescing produced only a doubtful result.

Concluding remarks.

This last series of experiments permits of only two interpretations.

Interpretation à la Pasteur: The KCl must contain germs which it is not possible to destroy even by repeated application of the usual sterilisation procedures. It remains to be explained how it is that these germs are not visible in the pure KCl and appear at the moment of admixture. In any case, the discovery of these germs would be of some interest.

Interpretation according to Reich's synthetic theory: The bouillon contains all the substances employed in the mixtures of the bion experiments, in particular the antithetical substances lecithin-cholesterol and potassium-calcium. It is thus understandable that adding potassium direct to the bouillon can have the same effect as that produced in the first mixture, either by enabling substances to organise or by restoring to them a vitality of which sterilisation had deprived them. Since the sterility of the KCl can only be tested by introducing it into the bouillon, there is no means of deciding between these two opinions.

Nice. 28 September 1937.

# THE HISTORY OF CONCEPTIONS

# OF THE ORIGIN OF ORGANIC LIFE

# SINCE THE 17th CENTURY/

by

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# THE HISTORY OF CONCEPTIONS OF THE ORIGIN OF ORGANIC LIFE SINCE THE 17th CENTURY.

## 1. Aristotle to Pasteur.

Until the 17th century it was quite generally believed that certain plants and animals, whose origin and development could not be traced directly, came into existence spontaneously out of inanimate matter.

Aristotle taught that every dry body that became moist, and every moist body that became dry, generated animals. Formulae for the production of mice and frogs out of mud and earth were still given in the 16th and 17th centuries, for example, by van Helmont. Such directions are to be found in Leeuwenhoek's Epistola, 75, 1692. Theologists of the 17th century who engaged in natural research (Kircher, Bonnani) taught that the lower animals must be capable of spontaneous generation, for the Bible does not mention that Noah took them with him into the ark. It is also related in the Bible that a swarm of bees sprang from the body of a dead lion.

But the number of living organisms for which spontaneous generation was assumed was gradually reduced more and more to the lowest animals and plants, and finally to the least organised single cells.

In 1648 Francesco Redi observed that no grubs appear in cheese or putrefying meat when flies are kept off with a gauze cover. He also claimed the existence of males, females and eggs in the parasites of humans and animals known in his time (Esperianza intorno alla generazione degl'insetti, 1660).

Leeuwenhoek (1632-1723) contested the genesis of Infusoria by infusion. He claimed to have observed conjugation amongst them. (Leeuwenhoek was the first to make microscopic observations of single celled organisms)

William Harvey (De generatione animalium, 1651) coined the dictum: Ovum est primordium omnibus animalibus commune.

In 1669 Jan Swammerdam developed a theory of evolution according to which all living creatures developed from germs, which were themselves living.

If microscopic observation destroyed primitive conceptions regarding spontaneous generation, they also provided an incentive to the development of new, experimentally based theories. Observation of the development of Infusoria brought

a series of researchers to the opinion that living matter preserves after its death a particular capacity for life, under the influence of which, in favourable conditions, it becomes re-united into a living organism, with a complexity of structure and organisation determined by these conditions.

Others believed, like Leeuwenhoek, that they could observe all kinds of sexual propagation in Infusoria.

In the middle of the 18th century the most important discussion of these views took place between two Catholic priests engaged on scientific research. Here for the first time experiment and counter-experiment played the most decisive r/ole in the argument.

In 1745 Needham published in London a work setting out his experiments, their results, and his conclusions. He had poured boiling meat broth into flasks, placed them in hot ashes to heat the air contained in them, and then firmly stoppered them. Since small animalcules nevertheless devel-:oped in the broth, he concluded that they must have been spontaneously generated after the boiling and heating had destroyed all possible pre-existent germs. Needham's experiments were highly respected. In 1747 he was made a member of the Royal Society in London, and later became one of the eight corresponding members of the Académie des Sciences. In 1749 he was the guest of the French scholar Buffon, with whom he later collaborated.

In 1749 Buffon published the first three volumes of his work on the generative process. In the second volume he analysed his theory of organic molecules and defended the hypothesis of spontaneous generation. Pasteur suggested that Needham's experiments had had a great influence on Buffon's ideas.

Needham, Buffon and their followers at first stood in complete opposition to Bonnet and his circle, who asserted th pre-existence of germs. (About this conflict Pasteur remarks that neither side were in possession of the truth and that this was a time "in which people loved to argue to exhaustion about systems and speculative viewpoints")

The first to control Needham's experiments was Lazzaro Spallanzani, in 1763. In 1765 he published in Modena a dissertation in which he opposed the theories of Needham and Buffon. In 1769 this work appeared in French, with counter-observations by Needham (who had probably himself instigated this translation).

Spallanzani undertook new experiments in reply to

Needham's criticism, which were published in Italian in 1774 and in French in 1777 (Opuscles de physique animale et végétale). The following are excerpts from the Needham-Spallanzani discussion:

"If", said Spallanzani, "the substances which have been put in the vessels and the air contained in them have been purified by fire, and care has also been taken to remove them from all contact with the surrounding air, and if, nevertheless, the flasks are found, when opened, to contain living animals, this would be a convincing disproof of the theory of generation from eggs. I do not even know what its supporters could reply to this."

In one of his comments on Chapter 10 of Spallanzani's first treatise, Needham writes:

"It only remains for me to discuss Spallanzani's last experiment, which he describes as the only one in his treatise which would seem to run contrary to my principles. He hermetically sealed 19 vessels, filled with various vegetable substances, and boiled them thus for an hour. But it is clear from the manner in which he treated his 19 vegetable infusions and subjected them to this torture, that he not only greatly weakened or perhaps totally destroyed the vegetable force of the infused substances, but that he entirely spoilt the small quantity of air remaining in the empty spaces of his flasks by the vapours and heat. Consequently, it is not surprising that the infusions so treated showed no signs of life. It was bound to happen.

"The following therefore, in a few words, is my last proposal and the result of my whole work. If he renews his experiments, let him use such substances as have been sufficiently boiled to destroy all presumed germs which are believed to adhere to the substances themselves or to the inner surfaces of the vessels or in the air within the same. Let him hermetically seal them without turning them over, so that a certain amount of air is left inside. Then let him dip them for a few minutes into boiling water for as long only as is necessary to boil a hen's egg hard and to kill the germs. In a word, let him employ any safety measures he likes, as long as he only seeks to destroy the presumed foreign germs, which come from without; and I reply that he will always find these microscopic creatures in a sufficient number to confirm my principles. If, after adhering to these conditions, and having left the vessels for the time necessary to generate these bodies, he finds nothing living and no sign of life on opening them, I will give up my doctrine and abandon my

views. This, I believe, is all that a discerning opponent can demand of me."

As a result of his renewed experiments, Spallanzani announced (Opuscles, Vol.1, Chap.3, p.39) that:

> "Half an hour's boiling was no obstacle to the development of Infusoria of the lowest order ... but after boiling for three quarters of an hour or a little less, no Infusoria appeared in the 6 infusions."

This controversy reached no conclusion. The question of the significance of atmospheric air, and its possible transformation by boiling, for the development and life of Infusoria, remained unsolved. Thus, as Pasteur remarked, Needham "had every right ... not to abandon his doctrine". Spallanzani, on the contrary, held to the opinion that only by his procedure were the pre-existent germs in the vessels destroyed, a factor with which Needham had not dealt adequately.

In 1774 Priestley discovered oxygen, and in 1777 Lavoisier found that life was accompanied by an uninterrupted process of oxidation. When Gay-Lussac now analysed the air in conserves prepared by Appert according to Spallanzani's procedure, he found:

> " ... that they no longer contain any oxygen, and that the absence of this gas is naturally a necessary condition for the preservation of animal or vegetable substances." (quoted by Pasteur, 1862)

Needham's anxieties regarding a decisive alteration of the air in Spallanzani's procedure were thus confirmed.

In the same treatise Gay-Lussac reported the following experiment:

> "I took cow's milk and heated it daily to the temperature at which salt-saturated water boils (109°C). Two months later it was still perfectly preserved."

In February 1837 Theodore Schwann published the following report. He had placed an infusion of muscle tissue in a glass flask through the stopper of which ran two bent tubes which dipped into baths of molten alloys, maintained at a temperature approximate to the boiling point of mercury. Care was taken that, after heating in the tubes, the air should cool before entering the vessel. In meat infusions which were boiled and thus treated, no change took place and no living organisms could be found.

The preservation of the conserves was therefore not

due to the absence of oxygen, as Gay-Lussac thought. Although Schwann obtained unambiguous results in his experiments on putrefaction, this was not the case with his fermentation experiments.

He filled 4 bottles with cane sugar solution mixed with yeast, closed them tight, put them in boiling water, and immediately afterwards placed them upside down in a trough of mercury. After they had cooled, he added ordinary air to two of them, and air which had been strongly heated to the other two. At first fermentation took place only in the bottles to which ordinary air had been added; in the others it failed to appear, even after two months. But repetitions of this experiment produced contradictory results which were at first inexplicable: at one time fermentation appeared in none of the bottles (e.g. after too long a time in boiling water), at another the fluid fermented even in the bottles to which heated air had been added.

Schwann concluded from his experiments:

"Alcoholic fermentation and putrefaction are neither of them caused by oxygen, at least not by the oxygen of atmospheric air, but by a principle contained in ordinary air and destructible by heat".

In the same year, 1837, Schwann and Cagniard de Latour discovered independently that the yeast of alcoholic fermentation consisted of unicellular organisms - "sprouting fungi"

Ure and Helmholtz confirmed Schwann's findings by analogous experiments in 1840 and 1844.

Franz Ferdinand Schulze, instead of heating the air, let it in through concentrated sulphuric acid and out through concentrated potassium hydroxide solution. No organisms appeared in the Appertian conserves thus treated; but in an open glass vessel containing the same substances standing nearby, they did appear.

H. Schroeder and Th. von Dusch, in 1854, developed an experimental procedure that went much further than the previous ones towards invalidating Needham's objection regarding possible chemical alterations in the air.

Francesco Redi had prevented the development of larvae in putrefying meat by covering it with a fine gauze. Others had greatly reduced the number of Infusoria in infusions by the same method. As early as 1743 Baker had drawn from such experiments the conclusion that:

" ... the eggs of these small creatures, which weigh

less than air, exist in millions in the air, and that a great number of them perish through being driven indifferently in all directions and settling in places alien to their natures ... "

In 1850, 1851 and 1853, H. Löwel published observations on super-saturated sodium sulphate solutions which indicated that ordinary air filtered through wood-wool had not the capacity to effect the crystalisation of this salt.

Schroeder and Dusch now carried out the following experiment. The organic substances were contained in a glass flask through the stopper of which ran two tubes bent at right angles. One of them was connected to a water aspirator, the other to a distant tube, 1 inch in diameter and 20 inches long, filled with wood-wool. After the tubes had been connected, the tap of the flask closed and the organic substance placed in it, the contents of the flask were brought to the boil and kept at this temperature long enough for all the connecting tubes to be strongly heated by the steam. The tap of the aspirator was then opened and it was left running day and night.

Both researchers carried out their experiments with the following substances:

1. Meat in water.
2. Brewer's wort.
3. Milk.
4. Meat without water.

1 and 2 remained unchanged, even after weeks. The milk curdled and putrified as quickly as in ordinary air. Meat without water quickly went putrid.

Schroeder and Dusch concluded:

"It therefore seems from our experiments that there are spontaneous disintegrations of organic substances whose commencement depends only upon the presence of oxygen: e.g. the putrefying of meat without water, that of the casein of milk, and the transformation of milk sugar into lactic acid (lactic acid fermentation). Besides these, however, there are other putrefaction and fermentation phenomena ... such as the putrefying of meat broth and alcoholic fermentation, whose development depends, in addition to oxygen, upon those unknown substances which are mingled with atmospheric air and which, in Schwann's experiments, are destroyed by heat and in ours by filtration ... Since there remain here so many questions that must be decided by means of experiment, we will refrain from drawing any theoretical conclusions from our experiments."

In a treatise published in 1859, Über Filtration der Luft in Beziehung auf Fäulnis, Gärung und Krystallisation, Schroeder announced further organic fluids which do not putrefy in filtered air: urine, starch-paste, and the various individual components of milk. He had ascertained that yolk of egg, however, like milk and meat without water, became putrid even in filtered air.

Schroeder posed the question: "Should this effective substance be looked upon as composed of microscopic organised germs, or is it a still-unknown chemical substance?" He remarked that the findings which he obtained relating to putrefaction and fermentation ran parallel to those concerning crystallisation. Air filtered through wood-wool lost part, but only part, of its putrefying and fermenting or crystallising power for certain organic substances and certain crystalline solutions.

Summarising Schroeder's findings, Pasteur remarked (in his work on "The organised bodies present in the atmosphere", that only "the presence in atmospheric air of an unknown principle" is demonstrated by experiments made so far as a pre-requisite of life in the infusions.

> "Those who asserted that this principle was nothing else than germs had no more proofs to support their opinion than those who thought it might be a gas, a fluid, miasmas, etc, and were consequently disposed to believe in spontaneous generation."

In the year 1858, F. Pouchet informed the Académie des Sciences of experiments with infusions which he had carried out, partly in collaboration with Houzeau, which seemed to him an irrefutable proof of heterogenous generation. (I shall describe one of these experiments in the course of the exposition of Pasteur's experiments). Pouchet was a corresponding member of the Académie, and his experiments received great attention. Milne Edwards, Payen, de Quatrefages, and Claude Bernard discussed the findings with Pouchet and finally the Académie set the following prize competition: "To attempt to throw new light on the question of spontaneous generation by means of successful experiments".

The committee (Geoffrey-Saint-Hilaire, Brongniart, Milne Edwards, Serres, and Flourens as returning officer) demanded "precise and rigorous experiments studied equally in all their details: in a word, experiments which give a definite and unambiguous result." (Meeting of 30th January 1860)

Louis Pasteur presented the major part of his experimental observations and conclusions on this question to the Académie

des Sciences at meetings on the 6th February, 7th May, 3rd September and 5th November 1860. He reported those which later comprised Chapter 2 of his treatise The Organised Bodies Present in the Atmosphere, that appeared in 1862, on the 4th May 1861 to the Paris Chemical Society (according to the German edition, on 9th December 1859).

His work on the question of spontaneous generation, contained in Volume 2 of the collected edition of his writings published from 1922 onwards, is generally regarded as the final refutation of assertions and theories regarding the spontaneous genesis of unicellular organisms from non-living substances. (Vide, e.g. Encyclopaedia Britannica, 14th edition, article on "Abiogenesis"; Handbuch der pathogen Mikroorganismen, 3rd edition, Vol.1, 1, pp.12-14, 1929; Handwörterbuch der Naturwissenschaften, 2nd edition, Vol.10, 1935, article on "Spontaneous Generation".) Hence they must be discussed here in greater detail.

Pasteur came to investigations of fermentation phenomena through his studies of molecular asymetry of organic (protoplasmic) substances. He found, in contrast to most of the other researchers, and in particular to Justus von Liebig,

> " ... that all fermentations proper - viscous, lactic acid, butyric acid, tartaric acid, and malic acid fermentation, and the fermentation of urine ... are in inverse ratio to the presence and development of organised creatures."

If, however, the fermentation stimuli were living organisms, there remained the question of their origin.

> "Of the two alternatives, only one is possible here. Since the ferments of fermentation proper are organised, they must have been generated spontaneously, if oxygen alone, as oxygen, calls them into being by its contact with nitrogenous substances. But if these ferments are not spontaneously generated organisms, then oxygen has no part in their formation, as oxygen, but merely as the stimulus for the development of a germ which is added at the same time as itself or was present in the nitrogenous or fermenting substances... At the point I had reached in my studies of fermentation, I had, therefore, to form an opinion on the question of spontaneous generation. Perhaps I should find in it a powerful support for my ideas on fermentation proper."

Pasteur began with a microscopic examination of the solid particles suspended in the air. Supporters of the theory of spontaneous generation, particularly Pouchet, had asserted

that "eggs of microzoa are occasionally to be found in the dust of the air", but that this was an exception. Starch or silica granules which are regularly to be found in the dust had been erroneously taken for microzoal eggs.

Pasteur took the view that Pouchet and others had been misled by the fact that they had only investigated quiescent dust, which was likely to consist of heavy mineral particles.

In order to be able to examine the free-floating dust of the air, Pasteur had air from streets or gardens drawn by an aspirator through a tube containing a plug of wood-wool. He carried out this filtration at various seasons, at various temperatures, and under various weather conditions, for one or two whole days at a time. He then dissolved the wood-wool plug in an ether-alcohol mixture and left the solution for a day to settle. The dust collected at the bottom of the tube and he washed it by decanting 5 or 6 times at intervals of 12 or 20 hours. The dust was then collected on a watch glass, the rest of the fluid quickly evaporated, and the dust, mixed with some water, could be examined under the microscope.

Pasteur found in the dust thus collected and prepared:

> " ... regularly a varying number of bodies .., whose form and structure indicate that they are organised ... in diameter of the most minute upwards to 1/100 or 1.5/100 or more mm. Some are completely spherical, others oval. The outlines appear more or less clearly. Many are absolutely translucent, but some are opaque with an internal nucleus. The translucent, clearly-outlined bodies resemble mould spores so closely that the most expert microscopist could see no difference. That is all that can be said of them, just as one can only state that amongst others there are some which resemble spherical and encysted Infusoria and, in general, those spherules which are held to be the eggs of these little creatures."

Pasteur ascertained the continual presence of starch granules by the iodine test. He separated them and the particles of chalk by mixing the dust with sulphuric acid (which does not dissolve mould spores).

He did not give the magnification he employed in figures but by the measurements mentioned it must have reached 200x.

He then proceeded to experiments on the inability of heated air to produce life, as asserted by Schwann and denied by Pouchet, Mantegazza, Joly, and Musset.

In a flask with a cubic capacity of 250 to 300 ml. he placed 100 to 150 ml. of the following mixture: 100 parts water, 10 parts sugar, 0.2 to 0.7 parts soluble protein and mineral substances from yeast.

The narrow neck of the flask was connected to a red-hot platinum tube. The fluid was boiled for 2 to 3 minutes and then allowed to become completely cold. The flask was filled with ordinary air by atmospheric pressure, but this air had first to pass through the red-hot platinum tube. The neck of the flask was then closed.

Pasteur repeated this experiment more than 50 times. He kept the closed flask for up to 18 months at a temperature of 25-30°C. In no case did the slightest trace of organic life appear in the fluid.

But such changes did take place when the same experiments were made in a trough of mercury.

In each of 5 flasks with a cubic capacity of 250 ml. Pasteur placed 80 ml. very clear yeast water containing 100 gm. sugar and 3 gm. soluble nitrogenous and mineral substances from the yeast. He drew the neck of the flask to a fine point in the flame, boiled the fluid for 2 to 4 minutes, and closed the end of the neck with a blow-pipe while boiling was still in progress. The flasks were placed upside down in a mercury bath and the ends of their necks were broken off on the bottom of the trough. Into the first he introduced about 70 ml. oxygen obtained from potassium chlorate, via a red-hot porcelain tube. To the second he added 50 ml. electrolytically produced oxygen. Into the third and fourth he passed ordinary air through a red-hot porcelain tube. Finally, to the fifth he added 50 ml. unheated ordinary air. He then placed the 5 flasks upside down on the mercury in an incubator at a constant temperature of 25-30°C.

After 5 days, organisms had appeared in all 5 flasks. Torulaceae in the first, sheets of mould in 3, 4 and 5. The second fell over in the night after 6 days because it had filled with gas as a result of fermentation. Microscopic examination of the fluid left in the flask showed yeast spheroids.

Pasteur carried out the same experiment with urine, milk and water containing sugar, white of egg and some calcium carbonate. With the two latter, weakly alkaline fluids, a temperature of 110°C. was needed to keep them free of organisms. But organisms were regularly found, as in the yeast water experiments, if the mercury procedure was used.

But if the mercury was first heated, the fluids remained sterile. If, on the other hand, a few drops of mercury were added to a flask whose contents had been treated according to the first method and had remained unaltered, bacterial cultures regularly appeared.

Pasteur concluded from this that ordinary mercury always contained dust, and therefore germs, and that during manipulations under mercury, dust was regularly and inevitably introduced. Consequently, he denied the validity of all experiments carried out in a mercury bath which purported to prove spontaneous generation, as, e.g. the following experiment of Pouchet:

> "A litre flask was filled with boiling water and, immediately after being hermetically sealed with the greatest care, placed upside down in a trough of mercury. When the water was absolutely cold, the flask was opened under the mercury and half a litre of pure oxygen added ... Immediately afterwards, a small bundle of hay was introduced into the flask, beneath the mercury, from a stoppered phial taken straight from a dry-oven in which it had been for 30 minutes at 100°C."

Eight days later a mould culture had developed in the infusion.

The oxygen was obtained from a chemical source, the water was boiled, the hay had been heated at 100°, and, after some objections had been raised, even at 200 and 300°, in fact until it was carbonised.

Pasteur replied that this was all very well, but that the error lay in the use of mercury. Such experiments simply could not be carried out under mercury. (Vallée, Oeuvres de Pasteur, Vol.2, pp.335-7)

To substantiate his assertions, in the course of a lecture on spontaneous generation at one of the Sorbonne scientific evenings, on the 7th April 1864, Pasteur carried out the following demonstration. He strewed the surface of mercury, in a 1 metre deep vessel, with dust so that it was entirely grey and matt. He then dipped a glass rod into it. Since mercury does not moisten glass, there was a space between it and the glass rod, into which the whole layer of dust was drawn. The surface of the mercury appeared metallically burnished again.

> "The whole of the dust is inside, in the lower part of the bath, and if I withdraw the rod the surface will again be covered with dust." (op. cit. p.339)

Further experiments of Pasteur's consisted in the inoculation of dust taken from the air in sterile infusions. He took one of his flasks which had stood for some months in an incubator at 25-30°C with its contents (yeast water, sugar and heated air) unchanged, connected its neck with a tightly fitting rubber tube to a wide glass tube in which lay a glass tube, open at both ends and containing a dust-laden wad of wood-wool. A T-tube with three taps was connected to this wide glass tube by a platinum tube which passed through a gas oven to an air pump. The platinum tube was brought to red heat, the tap to the T-tube closed, the space up to the still-closed neck of the flask was emptied, and then air was let in it through the red-hot platinum tube. After 10-12 repetitions of this procedure, Pasteur considered the wood-wool to be sufficiently impregnated with heated air, without having lost its dust. He broke off the end of the neck through the rubber tube, slipped in the tube containing the wood-wool wad, and reclosed the end of the neck in the flame.

The flask was replaced in the incubator. All experiments of this kind resulted in the development of mould, Torulaceae, and bacterial cultures after, at the longest, 48 hours.

To counter objections regarding the organic nature of wood-wool, Pasteur repeated the experiments with asbestos wads. Dust-laden wads produced cultures, those which were heated without dust produced none, and those which were dust-laden and subsequently heated produced none either.

Pasteur pointed out that the formations which developed in this way were the same which appeared in infusions exposed to the air, and that here, as there, the types which developed first and most powerfully prevented or suppressed the growth of the later or weaker ones. But in the inoculated infusions which are otherwise capable of fermentation, fermentation never appeared with his procedure.

In his "Studies on Beer" Pasteur states:

> "If it (the yeast) is provided with a sufficiently large quantity of free oxygen to ensure its life, its nutrition, its respiration - in other words, if it is allowed to live in the manner of all types of true moulds, then it ceases to be a ferment; that is, the relative weights of the plant and its principle carbon-containing food, sugar, are the same as those for mould growth. But if no air is allowed to reach the yeast, if it is left to develop in a sugar-containing medium from which free oxygen has been removed, it multiplies as though air were present,

though less actively, and then its fermentive character appears most clearly." ("Études sur la Bière", Oeuvres, Vol.5, pp.203-4)

As "another, very simple method of showing that all organised formations in infusions which have been previously heated owe their development to the bodies suspended in atmospheric air", Pasteur described the boiling of easily alterable fluids (beer-yeast water, sugar-yeast water, urine, beet sap, pepper water) in glass flasks whose necks contained multiple torsions. He let the fluids boil for a few minutes till the steam poured out of the end of the neck. He then allowed it to cool. The fluids showed "no other change than that produced, in certain cases, by direct, purely chemical, oxidation of the substances". Pasteur's explanation for this phenomenon was that while the air which first of all entered forcibly might carry germs as far as the infusion, these were destroyed by the almost-boiling hot fluid. And, "when the fluid is sufficiently cool to be no longer deadly to the germ the entrance of the air is sufficiently slowed up for it to leave all its dust in the moist torsions of the neck." According to Pasteur, this experiment was also successful with milk, so long as it was heated at rather more than 100°C under pressure, and only heated air was allowed to enter during the cooling. All such sterilised fluids were later subject to infection on the introduction of dust from the air.

Pouchet objected to Pasteur's assertions regarding the prevalence of germs in the air, that if this were the case the air must be laden with organic matter to the extent of forming a thick mist in it. Pasteur replied, firstly, that the multiplicity of bacterial types probably seemed greater than it really was, and secondly, that he had never asserted, or wished to assert, that the air is everywhere and at all times abundant in bacteria.

He boiled fluids capable of putrefying in flasks with drawn out necks, and closed the end of the neck over a flame during the boiling. After the fluids had cooled, he broke off the ends, so that air rushed in. He then closed the necks in the flame again and placed the flasks in an incubator at 25-30°C.

Usually a change took place in the fluids within a very few days in these experiments. The microbe growth was, in general, more profuse than in cases where the same fluids were freely exposed to ordinary air. Frequently, however - several times in each series of experiments - the fluid remained entirely unchanged.

From these experiments Pasteur drew the following conclusions:

"The germs, which are present in a smaller number in a limited volume of air, are not disturbed in their development by more numerous germs or germs which develop earlier, which may occupy the area and leave no room for anything but themselves. Thus only Penicillium glaucum, whose spores are vigorous and widely distributed, appeared in the course of a few days in unenclosed fluids, which, by contrast, showed very various formations when subjected to the influence of limited quantities of air."

But these experiments also showed him:

" ... that it is always possible to take from a given place at a given moment a considerable volume of ordinary air, which has not undergone any change, either physical or chemical, but is nevertheless completely incapable of producing Infusoria or Mucoraceae in a fluid which changes very rapidly and persistently on free contact with the air."

Pasteur supported this latter assertion by a series of experiments on the pattern just named (opening and re-closing of flasks whose necks had been fused during boiling) under various weather conditions, at various altitudes, in room (laboratory) and in quiescent air.

1. Two flasks were opened on a terrace, some metres above the ground, directly after a very short shower, and closed again soon afterwards. The first, which contained yeast water, showed a small sheet of mould after 8 days. The second, which contained yeast water and sugar, showed the same after only 2 days.

2. Four flasks (contents not specified) were opened on the same terrace after a heavy downpour with very large drops. Two flasks showed small sheets of mould on very clear fluid after 8 and 9 days respectively. The other two remained unchanged (observation was continued into 1861).

3. Of 6 flasks full of yeast water, opened in a room of the Pasteur laboratory, 4 remained completely clear, without the slightest trace of organised formations (opened on 20.7.1860, last observation date mentioned by Pasteur, April 1861). The other two yielded organised formations after 2 and 11 days respectively, one of them Infusoria and Torulaceae, the other a spheroid mycelium with a silky gloss.

4. On the 14th August 1860, 10 flasks with beer-yeast water were opened in the cellars of the laboratory; and 11 of the same kind on the same day in the courtyard of the laboratory

50 cm. above the ground, in a light wind. Those opened and re-closed in the cellar remained unchanged. Those from the courtyard yielded Infusoria, bacteria, Algae, etc., just like the other infusions brought into contact with limited quantities of ordinary dust-containing air.

5. Finally, 73 flasks were submitted to the influence of limited quantities of air at various altitudes. Twenty received air from flat land at a fair distance from any houses. Organised formations appeared in the contents (beer-yeast water) of 3 of them.

Of the 20 opened at an altitude of 850 metres, 5 infusions showed change, 15 showed none. Of the 20 flasks opened at an altitude of 2000 m. on Montauvert, near the Mer de Glace, in a fairly strong wind from the Bois Glacier, only a single one showed change (a Mucoracease).

Pasteur describes the precautions taken in these experiments as follows:

"To begin with I heat the neck of the flask fairly strongly in the flame of a spirit lamp. To introduce the air, I make a cut in the glass with the aid of a steel knife and break off the end, while raising the flask above my head facing the wind, with a pair of iron pincers whose two blades have just passed through the flame in order to burn the dust which might be present on their surfaces, part of which would unfailingly be driven into the flask by the rush of air."

In the first experiment on the Bois Glacier, Pasteur did not succeed in re-fusing the necks of the 13 open flasks because he was dazzled by the sunlit ice. He carried them back unclosed to the little inn on Montauvert, left them open in his bedroom during the night, and closed them the following mid-day. Of these 13 flasks, 10 developed Infusoria or moulds in their contents.

Pasteur, therefore, found in these experiments a new confirmation of the:

" ... basic thesis, that the first pre-requisite for the appearance of living creatures in infusions or in fermentable liquids does not consist of air, as such, but that it is to be found in the latter here and there in places, the distribution and frequency of which is in accordance with the hypothesis of germs."

He found a further proof of "the presence of spores of Mucoracease among the organised bodies which are so easily

discerned in the dust suspended in ordinary air" by "comparative investigations on the effect of temperature on the fertility of the spores of Mucoraceae and the germs suspended in the atmosphere". Following a communication by Duhamel on the capacity for germination retained by wheat grains heated at 110°C, Spallanzani carried out heat experiments on the seeds of higher plants and mould fungus spores. He "boiled the little grains which fall from the heads of ripe mould fungi", which Michelli had already regarded as the seeds of this plant, in water and poured this water over substances which easily become mouldy. Mould developed much more densely than on the same substances not thus moistened. But Spallanzani reported further ("Sur l'origine des petites plantes des moisisures", Opuscles, Vol.2.):

> "I have carried out the same experiment with dust that had been subjected to a much higher temperature than that of red-hot coal, and I have found that it does not deprive the spores of their capacity for reproduction."

These accounts, indefinite though they were, together with Payen's communication that the spores of Oidium aurantiacum were still capable of development after heating at 120°C, and his own discovery of the increased heat required to sterilise dilute alkaline fluids, led Pasteur to the following experimental procedure.

He slipped a piece of asbestos, which had been strongly heated shortly before, between the little heads of the mould to be investigated. Another, similarly sterilised, he allowed to become covered with dust from the air by the method already described. The piece of asbestos (or else the wood-wool wad) was placed in a small glass tube, open at both ends, which was placed in a previously heat-sterilised, wider U-tube, in which it could slide easily. The wider apparatus differed from that employed in the earlier inoculation experiments by the addition of a tube, filled with pumice stone moistened with sulphuric acid, between the platinum tube and the U-tube. The whole apparatus was emptied in the way previously described and filled with heated air. The U-tube was heated for a time in an oil, water, or salt water bath at temperatures of 100-132°C. Finally, the glass tube with the spore- or dust-laden wad was placed in the flask with a sterile infusion in the way already mentioned. Pasteur reported the following findings:

1. From air dust from a wood-wool wad, after being heated for 1 hour in a water bath at 100°C, placed in yeast water and heated air, there developed on the sides of the flask, after 3½ days, "a kind of dusty deposit which, during the following days, quickly covered the surface of the fluid. It is a colourless Mucoraceae with small, irregular, circular heaps in a slightly granular membrane". After a

further 5 days all development ceased and the membrane fell in tatters to the bottom of the vessel. Some 20 days later the flask was opened. Microscopic examination showed that the Mucoraceae, "like all Mucoraceae generally", was formed of granulations, which here, however, were relatively large. Their diameter was 0.002 mm. Along with them some very fragile Vibriones, which were no longer mobile, were to be found.

2. Air dust, heated for ½ hour in a boiling water bath, after 2 days on the surface of sterile milk (which had remained unchanged for 53 days) produced a translucent layer of serum. In the course of the following 2 days a yellowish, cheesy sediment collected on the bottom of the vessel. There was no development of gas. Microscopic examination showed two distinct types of bacteria in large numbers. "One type are threadlike, very mobile Vibriones, which move swiftly about by means of a quick, tremulous movement of the posterior half of their bodies. They are 0.006 to 0.009 mm. in length, and 0.0007 mm. in width. The other type are short, very much wider, somewhat coarctate, and often united in chains of 2 or 3. Each organism is 0.003 to 0.004 mm. long, and 0.002 to 0.003 mm. in diameter."

3. Dust from an asbestos wad, kept for ½ hour in a boiling water bath at 100°, after 2 days in yeast water containing suger and chalk, produced a "visible turbidity with a delicate membrane on all sides of the vessel", and after a further 2 days, "a milky turbidity with crinkled coagulations in the mass of the fluid and on the bottom". When the flask was opened, a vigorous evolution of gas took place on the same day - fermentation. The microscope showed two types of Vibriones. Pasteur was of the opinion that fermentation was evoked by the larger type, whose length was up to 0.01 mm. and more, and whose diameter was 0.0015 to 0.002 mm.; and that these were protected from contact with the air by the smaller aerobes of 0.0006 to 0.0008 mm.

4. Air dust (on asbestos), heated for ½ hour at 121°C in an oil bath, also produced mould mycelia , in yeast water after 2 days, in urine after 7 days. Development in yeast water proceeded rapidly and powerfully, in urine slowly and weakly. In the urine there appeared, after another 11 days, "another mycelium with flaccid hyphae which suffocated the first after a few days". Infusoria did not appear in these experiments.

5. Air dust, heated for ½ hour in an oil bath at 129°C, did not bring about the development of the slightest trace of

organised formations in yeast water. (At the time of the report 8 months had passed since the date of inoculation.)

6. Experiments with Penicillium spores showed that they retained their capacity for germination even after being heated for ½ hour at 119-121°C (oil bath); though only after 3½ days instead of after 1 day, as was the case with spores which had not been heated (or only heated to 100°); or after 2 days, as with Penicillium spores heated at 108°C. On the other hand, heating in an oil bath for ½ hour at 127-132° completely destroyed the capacity for germination in spores of Penicillium glaucum and Ascophora elegans.

Pasteur remarked in this connection that he had, of course, obtained no Penicillium glaucum from air dust heated at 120°C.

Finally, Pasteur undertook a further series of experiments which showed: "that the appearance of lower organisms does not necessarily presuppose the presence of organic plastic substances ..." The fact that "all experiments on spontaneous generation were carried out with infusions of vegetable or animal matter, i.e. with fluids containing sunstances that previously appertained to the organism ..." had been of essential significance for Needham's theories, for Buffon's "system of organic molecules", and for the corresponding conceptions of Turpin and Pouchet. Before communicating his counter experiments, Pasteur quoted the corresponding procedures of Buffon and Pouchet, and pointed out all that they have in common, both with each other and with the formulations of Turpin, Pineau and Laurent.

In opposition to them, he showed that "bacteria, Vibriones, Mucoraceae, etc." develop if dust from the air is inoculated, under the same conditions as in experiments already enumerated, in fluids compounded as follows:

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|---|---|
| Pure water | 100 |
| Crystallised sugar | 10 |
| Acid ammonium tartrate | 6.2-0.5 |
| Ash of beer yeast | 0.1 |

or in this fluid with the addition of 3-5 gm. pure calcium carbonate. In the second case "the same phenomena appear, but with a more marked tendency towards lactic acid, viscous, and butyric acid fermentation".

For comparison, Pasteur then inoculated spores of Penicillium or some other mould. They germinated easily in fluids which contained, in addition to

| | |
|---|---|
| Pure water | 1 litre |
| Crystallised sugar | 20 gm. |
| Ash of beer yeast | 0.5 gm. |

either 2 gm. acid ammonium tartrate (or alternatively, "a salt of ethylamins") or 1 gm. tartaric acid and 1 gm. potassium nitrate.

I shall refrain here, as elsewhere, from touching on Pasteur's additional more detailed accounts. A great number of works on micro-organisms which draw their nutriment exclusively from inorganic matter has appeared in the meantime. (Vide, W. Bavendamm's work on purple bacteria in Ergebnisse der Biologie, Vol.13, 1936.)

2. Cohn.

In the work most quoted here, Pasteur himself said about his experiments:

> "The whole progress of my investigations consists in having driven the supporters of the doctrine of heterogeneous generation into a corner."

And in 1886, four years later, in a lecture exposing the errors in A. Donné's experiments on spontaneous generation, he amplified this:

> "However, I hasten to repeat here what I have often said. It cannot be proved, a priori, that there is no spontaneous generation. All one can do is to show: 1) that there are unnoticed sources of error in the experiments, 2) that living organisms no longer appear when these sources of error are eliminated, without the basic conditions of the experiments themselves being altered." (Oeuvres, Vol.2, p.354)

But he was also capable, as in the lecture of 7.4.1864 already mentioned, of being stirred, when referring to the sterilisation of well-known fermentable fluids which he had carried out, to exclamations such as:

> "The doctrine of spontaneous generation will never recover from the fatal blow dealt to it by this simple experiment." (Oeuvres, Vol.3, p.342)

This was his real inner conviction, and his practical success in combatting diseases of vines, hops, silk-worms, animals and man, contained in his "Studies on diseases of silk-worms"

(Oeuvres, Vol.4) still further confirmed the belief that:

> "It is in man's power to drive parasitic diseases from the earth, if, as is my conviction, the doctrine of spontaneous generation is a chimera." (Appendix to "Études sur la Bière", Oeuvres, Vol.5, p.312)

But this practical significance of the conviction Omne vivum e vivo or Omnis cellula e cellula (Virchow) conflicted with the fact that spontaneous generation - now understood as the development of organic living matter from inorganic - was "an indispensable hypothesis" (Ernst Haeckel) to the theory of evolution, which had received a powerful new impetus from the work of Darwin. As A. von Tschermak correctly remarked, (Allgemeine Physiologie, 1924) in this way the question had been removed from biological research and had become the concern of natural philosophy.

Before I proceed to the most generally accepted of these hypotheses, I should like to cite a further series of experiments and experimentally founded conceptions on the question "spontaneous generation or pre-existence of germs?" which, largely stimulated by Pasteur's investigations, have contributed to a more detailed elaboration of the problem.

Ferdinand Cohn, who had published his first works on bacteria as early as 1853, was spurred by Pasteur's (and Hallier's) works to fresh, searching investigations, which enabled him, in 1876, to furnish the first correct description of endospore formation in bacilli and to throw new light on the question of the heat-resistance of these "permanent forms" (Before him, Petry (1852) and Pasteur (1865) had undoubtedly seen spores of this kind, but had not followed up their germination and development. Simultaneously with Cohn, Robert Koch succeeded in observing the formation and germination of endospores in Bacillus anthracis.) Cohn began with the publication of Beiträge zur Biologie der Pflanzen in 1870. Vol.1, No.2 to Vol.3, No.2 (1872-1880) contained his "Untersuchungen über Bakterien" (investigations on bacteria) carried out in collaboration with Eduard Eidam, Robert Koch, Dr. Miflet, Benno Mendelsohn and F. Neelsen. The following are some passages from these works relevant to our theme.

In the first work, which appeared in 1872, Cohn took up the cudgels against the conception of the "pleomorphism" of lower forms of life, supported by Hallier, Nägeli and others - a conception which has recently gained ground through Löhni's investigations. In the experiments which he undertook in this connection, on the question of "whether bacteria proceed exclusively from germs sui generis, or whether, as is

so often asserted, they can also spring from Penicillium and other fungi (such as Mucor mucedo)", he observed, like Burdon Sanderson before him, "that bacteria germs from the air rarely or never fall into chemical solutions, which are otherwise entirely suited to the development of bacteria, however long they are left open, but that mould spores do".

After summarising the counter-experiments from Spallanzani to Pasteur, in this work Cohn made the following statement regarding the whole question of spontaneous generation:

> "All the aforementioned works have a three-fold premise, 1) that bacteria are or may have been originally present in the water or the animal or vegetable tissues employed; 2) that these bacteria are destroyed by boiling; 3) that new bacteria fall from the air if this is not prevented by fusing the neck of the flask, by wood-wool wads, or simply by bending the neck of the flask downwards. Doubts can be raised against all these presuppositions."

Against point 3, Cohn cited the observations mentioned above. With reference to point 1 he said:

> "That bacteria are already present in all organic matter, e.g. in boiled white of egg, a freshly-peeled seed or in the blood or flesh of a newly killed, healthy animal, contradicts the experiments of Burdon Sanderson referred to above. On the other hand, it is true that all water contains bacteria, though often only in small numbers and without propagation, perhaps a condition of cellular quiescence or permanency."

He had obtained various results from experiments relating to point 2, in which he heated bacteria in an artificial nutrient fluid or together with animal or plant tissues.

Bacteria heated in an artificial nutrient fluid (100 parts distilled water, 1 part acid ammonium tartrate, and about 1 part carbon-free yeast ash), without exception, died after 1 hour's heating at 60-62°C. But the results obtained when animal or vegetable tissue (usually a pea, previously boiled in distilled water and then shelled) were added to the nutrient fluid were entirely un-uniform. On one occasion, development of bacteria was eliminated by heating at 80° for ½ hour, on another it appeared rapidly and definitely after the fluid was heated for even longer periods at the same or higher temperatures. Cohn sought an explanation for this in the fact that the bacteria which had entered these poor heat conductors were out of reach of the high temperatures of the fluid.

Cohn had already noticed during these experiments that Bacillus subtilis showed greater resistance than Bacterium vulgare. In 1873 he came to the conclusion that this was probably explained by the capacity of bacilli to form resting spores, which had already been assumed by Petry, Trecul and Billroth.

In 1872 Henry Charlton Bastian had boiled a small quantity of cheese in a turnip decoction, filtered and neutralised the fluid, and boiled it again for 10 minutes in a flask, the neck of which he fused while the fluid was still boiling. After 3 days the fluid was seething with bacteria. Cohn repeated the experiments with extreme precautions.

> "A small flask, the neck of which had been drawn to a small open tip, was filled with distilled water and brought to the boil on a wire gauze over a gas flame. On the same gauze stood a beaker with the filtered turnip decoction. After both had been kept boiling for 10 minutes the flask was dipped in the beaker upside down with the tip in the fluid, and the flame was removed. The steam generated in the flask first forced the remaining water into the boiling decoction. But as it cooled the latter rose into the flask. As soon as this was full it was taken out and the end immediately fused."

Cohn found Bastian's findings confirmed. But he was not satisfied with Burdon Sanderson's demonstration that this success was not achieved if the decoction was heated at rather more than 100°C in a Papin's digester. He obtained rennet from a Swiss cheese dairy and made liquid extracts from it. On microscopic examination he observed that the fresh extracts only contained the thin, motile bacilli which others (Remak, Wedl) had observed before, but that after some time a continually increasing proportion of the rods showed "capitate swellings" at one, or more rarely at both, end which were filled with an oval or spherical, strongly refractive body. From these and similar observations on bacilli, Cohn interpreted the results of the Bastian experiment as a demonstration that it was these spores which germinated into new bacilli after boiling.

This opinion became his firm conviction when, in 1875, he succeeded in following under the microscope endospore formation in Bacillus subtilis from boiled hay infusions.

I shall repeat here Cohn's essential conclusions from his observations (Vide, Beiträge zur Biologie der Pflanzen, Vol.2, p.256 et seq.):

1. In boiled fluids neither Bacterium vulgare nor any other

known microscopic organism developed, with the exception of bacilli ...

2. If bacilli develop in the boiled infusions their origin is to be sought in their developmental history as already known to us. We cannot doubt that bacillus spores are contained in the hay ... In dry hay these spores are shrunken and difficult for water to soak into, and during infusion they are washed into the liquid. But as long as the spores are not soaked with water and swollen they can be heated at 100°C without loosing their capacity for germination; as has been demonstrated not only for fungus spores, but even for phanerogam seeds, under the same conditions. That unswelled bacillus spores can remain in boiling water for at least 15 minutes, and in individual cases for even 1-2 hours, without being killed, probably depends upon their oleaginous content, and perhaps also upon a layer of air adhering firmly to the spore which may evoke Leidenfrost phenomena; in part it depends upon individual peculiarities ..."

According to Cohn's investigations, hay bacilli do, indeed, lose all capacity for propagation and development, including spore formation, at 50-55°C. But existing spores occasionally survived 3 to 4 days' heating at 70-80° without losing their capacity for germination. In the work under discussion Cohn reserved the task of "establishing more exactly the temperature range within which development of bacilli takes place, for subsequent investigations to be based on more exact methods". I do not know whether he carried out this proposal, but it has been undertaken by a series of other researchers and I shall survey their findings in the appexdix.

In Vol.3, No.1. p.149 et seq., Cohn and Miflet finally reported their "Untersuchungen über die in der Luft suspendierten Bakterien" (investigations on air-borne bacteria). They succeeded, through improvement of the "air-scrubbing" procedure (the suction of air through sterilised nutrient fluid) previously employed - unsuccessfully - by Cohn, in obtaining bacteria cultures, whose origin had to be ascribed to germs floating in the air.

The washing fluids were of three kinds:

1. A mineral nutrient solution of the following composition:

| | |
|---|---|
| Potassium acid phosphate | 1 gm. |
| Magnesium sulphate | 1 gm. |
| Neutral ammonium tartrate | 2 gm. |
| Calcium chloride | 0.1 gm. |
| Distilled water | 200 ml. |

2, A ten per cent solution of malt extract.

3. A one per cent solution of Liebig's meat extract.

For "absolutely certain disinfection of the apparatus when they had been filled with meat and malt extract" it proved necessary to boil them for 1½ hours in a Papin's digester. The mineral solution was sterile after only 20 minutes.

Preliminary experiments showed that:

> " ... germs contained in the air were completely removed by filtering through cotton wool, and that the appearance of bacteria during the use of a filter of ordinary cotton wool was attributable to the impurities contained in the latter and dislodged by the strong stream of air. In consequence of this finding, a cylinder in which the air was filtered through disinfected cotton wool was attached every time for purposes of control. If this remained clear, while the other cylinder, in which air was not filtered, clouded, it could be assumed with complete certainty that the germs which developed in the washing fluids must derive solely and exclusively from the atmosphere."

Cohn and Miflet summarised the conclusions to which the were led by their experiments in the following words:

1. Numerous bacteria germs, capable of developing, are ~~are~~ suspended in the air.

2. By the methods we employed, these germs can be collected, brought to development and propagation, and consequently also systematically differentiated and identified.

3. The presence in the air of germs, capable of development, of a wide variety of types of bacteria, in particular of Micrococci and Baccili. is already proven by our method. For the major part, the same had already been discovered previously in other media. One group of very peculiar forms had not hitherto been definitely identified.

4. On the other hand, the presence in the air of germs of very many bacteria which usually develop in fermenting substances could not be demonstrated. This applied particularly to Bacterium vulgare, Spirilla, Spirochaeta, and many others.

5. The presence of bacteria germs in air drawn up out of the ground has been proved in individual instances.

6. By contrast, the air of a crowded sick-room of a typhus hospital proved to be free from bacteria germs capable of development, probably in consequence of effective ventilation and disinfection.

7. The air rising from a sewer was rich in bacteria germs capable of development.

8. The number of observations and experiments made in this first systematic investigation is not sufficient to establish whether a pathogenic significance is to be attributed to the variety of the bacteria collected from the air in various places. But experiments have so far given a negative result."

3. Tyndall.

The numerous experiments carried out by John Tyndall, at the same time as Cohn's, for the purpose of refuting assertions regarding spontaneous generation, which Henry Charlton Bastian had at that time begun to expound with particular vigour in England, did not add anything fundamentally new to the experiments already reported. Of his alterations and modifications in the experimental apparatus, the only one which seems to me worthy of remark is that designed to ensure dust-free air.

It consisted of a wooden box which had one longitudinal wall of glass, while the opposite one contained a door which was dust-proof when closed. In both transverse walls, glass windows faced each other, and a series of test tubes could be inserted dust-tight in the floor. Contact with the outer air was maintained by two tubes, running through the top and twisted into several S-bends above it. Through a rubber ring in the top passed a long pipette with a funnel at the upper end, the tip of which reached the test tubes set in the floor. Finally, the inner walls of the box were covered with a layer of glycerine. (As far as I can gather, this last detail was derived from Pouchet, whose name has already been mentioned). If a strong beam of light, directed through the side window, was invisible inside the box, indicating that the air was free from dust, the test tubes were filled via the pipette. After heating the test tubes, Tyndall placed a cotton wool wad over the upper end of the S-shaped tubes, so that the air drawn in during cooling should not bring dust into the box with it.

(Since Tyndall was the first to evaluate in systematic experiments the visibility of ultra-microscopic particles

suspended in gases or fluids, the phenomenon is known, after him, as the "Tyndall effect". Upon it Siedentopf and Zsigmondy developed ultra-microscopy.)

Tyndall investigated in this way infusions of a very large number of animal and vegetable tissues, and obtained entirely similar results to Pasteur.

4. Huizinga.

Another controversy over the question of spontaneous generation from the same period should be mentioned. It was published in Pflügers Archiv für die gesamte Physiologie, Vol.7-11, in the years 1873-1875.

D. Huizinga announced here a series of experiments which he had carried out as a supplement to Bastian's turnip-cheese experiment already mentioned, which seemed to him to prove incontrovertibly the possibility of demonstrating artificial spontaneous generation - he used the expression "abiogenesis" introduced by Huxley in 1870.

I shall detail here only the conclusion of this debate, since Huizinga himself finally maintained the validity of only a part of his experiments, against immediate and energetic contradiction from Burdon Sanderson, Paul Samuelson, Richard Gscheidlen and Felix Putzeys. This series of experiments was remarkable for the fact that - as far as I can see - it was the first occasion on which the use of infusions of animal and vegetable substances was renounced. I shall reproduce here, from Pflügers Archiv, Vol.8, 1874, p.564, the table in which Huizinga set out the experiments and control experiments which seemed to him decisive.

The "salt solution" referred to consisted of:

| | |
|---|---|
| Potassium nitrate | 1 gm. |
| Magnesium sulphate | 1 gm. |
| Neutral calcium phosphate | 0.2 gm. |
| Distilled water, | 500 ml. |

The "pure amylum" we should to-day call "erythrodextrin".

To close the flask Huizinga employed finely porous clay discs cemented with asphalt-lacquer.

| Experiment. | Result. | Conclusion. |
|---|---|---|
| 1) A mixture of 2 parts glucose, 0.3 amylum, 0.3 peptone, and 1 neutral ammonium tartrate, in 100 of salt solution. was heated for 10 minutes at 100°C. Then neutralised with CaO. Closed. Incubated. | Full of bacteria after 2-3 days. | |
| 2) As 1) but without ammonium tartrate. Closed. Heated for 10 minutes at 102 or 104°. Incubated. | Full of bacteria after 2-3 days. | |
| 3) 1 part glucose and 1 ammonium tartrate, in 100 of salt solution. Heated for 10 minutes at 100°. Closed. Incubated. | No bacteria after 8 days. | The bacteria obtained in 1) did not develop from germs introduced with the salt solution or the glucose. |
| 4) 0.3 parts amylum, 0.3 peptone, 1 ammonium tartrate, in 100 of salt solution. Heated for 10 minutes at 100°. Closed. Incubated. | No bacteria after 8 days. | Neither from germs introduced with the amylum or peptone. |
| 5) 1 part cane sugar. and 0.5 urine, in 100 of salt solution. Boiled. Cooled. 3 drops of a fluid containing bacteria were added. Closed. Incubated. | Turbid and full of bacteria after 1-2 days. | Cane sugar-urine solution is a fluid perfectly adapted to the nourishment of bacteria. |
| 6) As 5) but with ammonium tartrate. Heated for 10 minutes at 100°. Cooled. Infected as 5). Closed. Incubated. | Turbid and full of bacteria after 1-2 days. | Introduced bacteria develop perfectly well in an acid fluid. |
| 7) 1 part cane sugar, 0.5 urine, 1 ammonium tartrate, in 100 of salt solution. 3 drops fluid containing bacteria | No bacteria after 8 days. | A temperature of 100° for 10 minutes in an acid solution destroys bacteria. |

| Experiment. | Result. | Conclusion. |
|---|---|---|
| added. Heated for 10 minutes at 100°. Closed. Incubated. | | |
| 8) 1 part cane sugar, and 0.5 urine, in 100 of salt solution. 3 drops bacterial fluid added. Closed. Heated for 10 minutes at 102°. Incubated. | No bacteria after 8 days. | A temperature of 102° for 10 minutes in a neutral solution destroys bacteria. Th bacteria obtained in 2 did not develop from germs present in the fluid. |
| 9) 1 part cane sugar, and 0.5 urine, in 100 of salt solution. Heated for 10 minutes at 100°. Closed. Incubated. | No bacteria after 8 days. | The clay discs do not let through any germs from the air. (This conclusion also follow from 3),4),7) & 8). |

General conclusion: Under the experimental conditions outlined, organisms can develop without the direct intervention of pre-existent germs."

The arguments of the above-named researchers against Huizinga were based, as usual, on the objection that it was by no means proven that the germs of living organisms were destroyed by boiling heat in all fluids. (It is to be borne in mind here that the discovery of the endospores of bacilli by Cohn and Koch had not yet been made at the time of this debate!) I shall quote some passages from the articles directed against Huizinga:

"It is just the same with yeast, which, in a moist condition, is killed by slow-heating at 84°, while individual cells survive 5-10 minutes at boiling point. But all the yeast cells die after being boiled for ½ hour. This was also observed by Wyman and Hofmann in relation to bacteria. When Wyman boiled meat broth for 2 hours, bacteria always appeared, but when he boiled it for 5-6 hours, no organisms developed (American Journal of Science and Arts, 44, 152). Hofmann (Bot. Zeit, 1863, p.317) found that, with increased steam pressure, a few minutes were enough to produce the same result ... Lex found Vibriones still alive after the addition of some drops of urine in a state of alkaline fermentation to a hot saturated solution of sodium nitrate at 117°C (Klin. Wchschr.,1867, p.403. In the Vierteljahrschr. f. öff. Gesundheitspflege, 4, p.62, the same author states 127°). According to Schroeder, germs in meat, egg-yolk and milk

are only killed at 130°. According to Place (Maandblad voor Natuurwetenschappen. 1873, p.126) bacteria are only destroyed at 160°. According to Grace-Calvert (Proc. Roy. Soc., London, Vol.19, p.475), a temperature of 244° is necessary for the destruction of all bacteria ..."
(R. Gscheidlen, Pflügers Archiv, Vol.9, p.116).

It is interesting that all Huizinga's opponents named refused to work with the clay disc closure introduced by him. But they made the addition of acid possible, either by Pasteur's method, through tubes containing several torsions which, in some cases, had their ends closed with disinfected cotton wool or, like Putzeys (Pflügers Archiv, Vol.11, p.397), by means of thin glass ampoules filled with acid which were placed in the vessels and after heating were broken inside the fused flasks by shaking. In addition, Putzeys demonstrated that the acid in the tubes had by no means disappeared after the experiment.

Furthermore, Huizinga reported in his fourth and last communication (Pflügers Archiv, Vol.10, p.62 et seq.) that he had, with equal success, replaced arylum by the more easily purified inulin. The peptone should in no event be alkaline, but rather slightly acid.

With regard to the dependence of heat-resistance of bacteria on the reaction of the medium, Huizinga came to the opposite conclusions to Pasteur. According to him "they are least resistant in an alkaline fluid; in acid they resist high temperatures somewhat better; and in neutral solutions best of all". F. Cohn, on the contrary, found ~~that~~ in boiled hay infusions no constant difference in resistance between acid and neutral media.

Huizinga denied the validity of heating his mixture to 110° as a disproof of de novo development of bacteria, since peptone is chemically altered at this temperature. But he regarded as complete proof of the sterility of his mixture the fact that:

" ... bacteria are absent when the glucose or the inulin are omitted from the mixture, although lack of both these substances does not reduce the nutritional value of the fluid for bacteria."

In relation to the difficulties which undoubtedly resulted from this "veto" on higher temperatures, it seems to me worth emphasising that Huizinga recognised the bacteria which appeared in his experiments as Bacterium vulgare, Micrococcus and Vibrio. All new experiments known to me showed that the destruction temperature of all these types is below 100°C - including Vibriones, contrary to the remarks of Gscheidlen quoted above.

5. Bastian.

Henry Charlton Bastian championed the possibility of the spontaneous development of bacteria from disintegrating fluids from 1870 onwards, but I have not referred to him more closely till now, firstly because he maintained his opinion and the validity of his experiments till his death in 1915 and, secondly, because he seemed to have been the first researcher who called upon the theory of evolution in support of his views. Unfortunately, I have not been able to gain access to his last books, Studies in Heterogenesis and Nature and Origin of Living Matter, which appeared in London in 1903 and 1905 respectively, or his speech to the International Medical Congress in London in 1913, so I must confine myself here to some references to what he published before 1874.

Bastian distinguished between heterogenesis - the transmutation of living organisms or parts of living organism into other living organisms - and archebiosis - the development of living organisms from non-living substances. Most of the researchers of that period were in agreement with Bastian in the assertion that the possibility of both phenomena must be inferred from the theory of evolution, insofar as they accept this theory, but most of them looked on archebiosis as a phenomenon of an earlier period of the earth's history. But even those who regarded the conditions for the genesis of life as being still present to-day, generally rejected the validity of Bastian's experiments. Thus it can be understood that Bastian, after early successes soon stood pretty much alone and disregarded, in opposition to the authority of Pasteur, Tyndall, Cohn, Koch, Charles Darwin (who, in a letter to Wallace in 1872, declared that Bastian's statements were unworthy of belief, although archebiosis was probable), Huxley and Spencer.

Maybe Bastian himself was not always the best advocate of his own case - perhaps just because the situation was so unfavourable to him. For this very reason I have endeavoured, as far as possible, to be even more strictly objective in the following - as stated, unfortunately very incomplete - survey of his arguments and analyses, than in the other sections of this work.

Those of Bastian's writings with which I have become acquainted (Modes of Origin of Lowest Organisms, 1871; The Beginnings of Life, 1872; Evolution and the Origin of Life, 1874) are to a large extent filled with discussions of the evolutionary theory. In these he attacked Huxley, who considered that the conditions which had made archebiosis possible in remote periods of time were no longer present to-day, without being in a position to make any concrete state-

:ment about these conditions. The same reproaches were made against Tyndall and Spencer. He addressed to the latter in particular the question of how he could regard as compatible with his evolutionist views the opinion that, crudely expressed, Amoeba had remained Amoeba since primordial times, although simultaneously an uninterrupted line of development led from just the same undifferentiated masses of protoplasm to man? Only because Spencer was disinclined to assume the archebiotic or heterogenetic development of the simplest forms of life, was he forced to seek refuge in the hypothesis that such structurally stationary organisms had simply always succeeded in "evading" the changes in environmental conditions which had forced other proto-organisms into the path of differentiation and association. Even without any experiment:al support, as pure hypotheses (assumed as continually repeated processes), archebiosis and heterogenesis fitted the idea of evolution far better.

Further, obviously no-one would deny that inorganic matter is constantly being transmuted by plants, and dead organic matter by animals, into living matter. How, then, can such a transmutation as a spontaneous phenomenon, independent of pre-existent life, be declared *a priori* impossible, or at least relegated to some distant past, unless one begins from theological or vitalistic premises?

Bastian lays the main stress on the acceptance of the constant possibility of archebiosis, which would automatically bring with it that of heterogenesis - asserted on its own by Needham, Buffon and Pouchet.

Bastian ridiculed blind belief in the formula *Omne vivum e vivo* by reference to an example of careless generalisation from experience from the younger Mill. Until the discovery of black swans, the proposition "All swans are white" must have seemed to Europeans "an unequivocal example of uniformity in the process of nature", since no contradictory observation was available from all the past centuries.

I have not found any contribution to the argument as to the impossibility of deriving psychic life from the phenomena of non-living nature in the writings of Bastian's named above. Conclusions *a posteriori* regarding his attitude to this question are, however, possible through the fact that in 1880 he published a book entitled The Brain as an Organ of the Mind, which was described in the British Universities Encyclopaedia, Vol.1, 1935, as "the most complete exposition which has yet appeared of the views, represented by the extreme physiologic:al school, on the subject of psychology".

Whether religious motives also played a part in the

differences of opinion in relation to Bastian's experiments, assertions and theories, as in the Pasteur-Pouchet case, I cannot say. It seems to me not without interest that Tyndall's writings, which were essentially directed against Bastian, were published in France by the Abbé Moigno, as a scientific witness for the fact that "the cell and the molecule are of a nature known only to God", and "that God, after creating all that he wished and found necessary, had retired for the whole duration of the world into his immutabl peace". For the same reasons, the Catholic party in France had vigorously defended Pasteur (himself a devout Catholic), while the Liberals were favourably disposed towards Pouchet, who championed the "philosophical necessity of spontaneous generation" against the dogma of creation to which Pasteur was bound.

Neither Tyndall, Pasteur or Cohn ever seem to have answered Bastian's theoretical arguments; whether this was done from any other quarter is outside my knowledge. Tyndall refered to them briefly as "not new, but very cleverly present:ed", and then declares that he considered it useful to test Bastian's experiments, but to pass over his theories and critiques.

Bastian was a supporter of the chemical theory of fermentation, principally represented by Liebig. Like Pouche and his friends before him, he was inclined to see in the fermentation organisms, products rather than producers of th fermentation phenomena. For instance, he criticised Pasteur' experiments with inoculated dust on the ground that the latt had, in this way, introduced into the experimental fluids "a large quantity of what his scientific opponents regarded as non-living ferments, while possibly a certain quantity of living ferments existed in it". (Evolution and the Origins o Life, p.113, footnote.) In a letter to Tyndall, which seemed to refer to other works of Bastian unknown to me, Pasteur rejoiced over the fact that he seemed to have forced Bastian into a position which he, Pasteur, described in 1862 as follows:

> "'There are present in the air,' he will say, 'solid particles such as calcium carbonate, silica, soot, filaments of wool and wood-wool, starch, ... and also organised bodies exactly resembling the spores of Mucoraceae or the cysts of Infusoria.
>
> But I prefer to attribute the origin of Mucoraceae and Infusoria to the former amorphous bodies rather than to the latter.'"

I do not know whether Bastian still maintained his categoric assertion that all bacteria and also (at that time still hypothetical) bacteria germs were destroyed in a few minutes by damp heat at 140°F (60°C) and that dry heat achieved the same result at even lower temperatures, after Cohn's and Koch's discoveries. In those of his works known to me, at any rate, he held to this and reported long and manifold experiments intended to prove it.

I have already spoken briefly of that one of Bastian's main experiments which met with the greatest public recognition, while discussing Cohn's work. (It was this experiment which stimulated Huizinga to make his own experiments.) I shall now give the two further experiments to which Bastian, in Evolution and the Origins of Life (pp.175-178), attributed the greatest validity:

"Experiment I. A strong infusion of turnips was rendered faintly alkaline by liquor potassae, and to this a few separate muscular fibres of a cod-fish were added. Some of this mixture was introduced into a flask of nearly two ounces capacity. Its neck was drawn out and afterward hermetically sealed by the blow-pipe flame, whilst the fluid within was boiling. When thus closed the flask was about half full of fluid. It was then introduced into a digester which was gradually heated, and afterwards kept at a temperature of 270-275°F (132-135°C) for twenty minutes, though it seems also well to point out that if we include the time taken for the water of the digester (in which the closed flask was immersed) to attain this heat, and also again to cool down to 230°F (110°C), this flask was exposed to temperatures above 230°F for one hour, as I myself carefully noted at the time. When withdrawn from the digester the closed flask was kept at a temperature of 70-80°F (21-27°C) for eight weeks, and during part of this time it was exposed to the influence of direct sunlight. After it had been ascertained that the flask was free from any crack or fault, its neck was broken in order that its contents might be examined. The reaction of the fluid was found to have become decidedly acid, and it had a sour though not foetid odour, as though a fermentative process had been taking place in the solution. The fluid was very slightly turbid, and there was a well-marked sediment consisting of reddish-brown fragments, and of a light flocculent deposit. On microscopic examination the fragments were found to be portions of altered muscular fibre whilst the flocculent deposit was composed ~~of~~ for the most part of granular aggregations of Bacteria. In the portion of fluid and of deposit which were examined, there were thousands of Bacteria of most diverse shapes and sizes, either separate or aggregated into flakes. There were also a large number of monilated chains, of various

lengths, of a kind very frequently met with in abscesses and other situations where pyaemia or low typhoid states of the system exist, in the human subject. There were, in addition, a large number of Torula corpuscles, as well as of brownish nucleated spore-like bodies, about 1/30,000th up to 1/2,500th of an inch in diameter. Lastly there was a small quantity of a mycelial Fungus filament, bearing short lateral branches, most of which were capped by a single spore-like body.

Experiment II. A strong infusion of common cress (Lepidium sativum), to which a few of the leaves and stalks of the plant were added, was inclosed in an hermetically-sealed flask in the same way, heated in the digester at the same time (and therefore to the same temperature) and was subsequently exposed to the influence of the same conditions as I have already mentioned in connection with the last experiment. This flask was, however, opened one week later - that is at the close of the ninth week after it had been heated in the digester to 270-275°F (132-135°C). Before breaking the neck of the flask the inbending of the glass under the blowpipe flame showed that it was still hermetically sealed. The reaction of the fluid was found to be distinctly acid, though there was no notable odour. The fluid itself was tolerably clear and free from scum, but there was a dirty-looking flocculent sediment at the bottom of the flask, amongst the débris of the cress. On microscopic examination (with a 1/12th inch "immersion" objective) much altered chlorophyll granules existed, either dispersed or aggregated amongst the other granular matter of the sediment, and amongst some of this three minute Protamoebae were seen, varying in form, and creeping with moderately rapid slug-like movements. They contained no nucleus, and presented only a few granules in their interior. In the same drop of fluid, and also in others subsequently examined, more than a dozen very active Monads (1/4,000th of an inch in diameter) were seen, each provided with a long, rapidly-moving lash by which neighbouring granules were freely knocked about. There were many smaller, motionless and tailless spherules of different sizes, whose body substance presented a similar appearance to that of the Monads - and of which they were, in all probability, earlier developmental forms. There were also several unjointed Bacteria, presenting most rapid progressive movements accompanied by quick axial rotations. Many Torulæ corpuscles and other Fungus "spores" also existed, as well as portions of a mycelial filament containing equal segments of colourless protoplasm within its thin investing membrane.

A drop of the fluid containing several of these

active Monads was placed for about five minutes on a glass slip in a water oven maintained at a temperature of 140°F (60°C). All the movements of the Monads ceased from this time, and they never afterwards showed any signs of life."

In this regard Bastian said: "Nothing that has yet been alleged by way of objection to the admission of 'spontaneous generation' as an everyday fact, at all affects such experiments as these." But it can easily be seen that, quite contrary to Bastian's opinion, the validity of these experiments collapses completely as soon as the assertion that no living organisms can withstand temperatures above 60°C for more than a few minutes is refuted.

"In investigations of this kind a positive result has been achieved when no organisms develop, a negative result when they do", said Pasteur at the conclusion of his debate with Bastian. This proposition shows the complete hopelessness of a clear decision in this conflict. What is required by the one for the "real destruction of, or protection from, all germs", means to the other the "destruction of the preconditions for spontaneous generation". Only a "theory of life" which can explain all the findings of these pro- and contra-experiments without residium would open a way for a final decision.

How long and how far Bastian defended the validity of the experiments mentioned, I do not know. According to a remark of Tyndall's, he seems, at least up to 1877 or 1878, to have given no reply to the refutation of his experiments which Tyndall published in the Philosophical Transactions, 1876. Later still, Tyndall himself encountered quite particular difficulties in sterilising infusions of the kind employed by Bastian. Only after he had had his box, described above, constructed of tin instead of wood, and had taken particular precautions regarding the place in which he made the experiment in order to obtain especially still and dust-free air, so that no air-bubbles could enter the chambers while the infusions were being filled via the funnel-pipette, could he keep the decoctions of turnip and melon regularly free from infection.

From July 1876 to July 1877 Bastian had a debate with Pasteur on the genesis of bacteria in boiled urine which had been neutralised by means of potassium hydroxide and kept in dust-free air at 50°C. (Vide, Pasteur, Oeuvres, Vol.2, pp.463-473). Pasteur demanded that Bastian should use either solid potassium hydroxide which had been incandesced, or at least heated at 110°C, or else a KOH solution which had been kept for 20 minutes at 110°C. Bastian finally accepted this condition (in the second form). Since organisms still formed

on one occasion and not on the others, Pasteur declared these results to be due to variations in the effectiveness of the cleaning of the vessels. He reported that Bastian had repeatedly admitted to him that he had not taken account of this detail.

For the counter-experiment, which was carried out before a commission of the Académie des Sciences, consisting of Dumas, Boussingault and Milne Edwards, Pasteur gave the following directions:

"Place the urine in a vessel which has been burnt out with a flame and closed with a cotton wool plug while cooling.

40-50 ml. urine are kept in boiling water for 10 minutes.

After it has cooled, determine the acidity of the urine. Place a volume of potassium hydroxide solution equivalent to 15 ml. urine, whose acidity has been determined, in the glass vessel by means of the fusiform attachments, as illustrated, which have been burnt out. Close the vessel with a blow-lamp above the cotton wool wad. (See diagram)

Heat the vessel for 10 minutes in a bath of calcium chloride solution. Let it cool and wash the outside of the vessel to remove adhering calcium chloride.

Open the vessel above the cotton wool. Draw 17-18 ml. urine into the part containing no potassium hydroxide.

Immerse for 10 minutes in boiling water at 100°C. Allow to cool off.

Place 15 ml. urine into the part containing potassium hydroxide. 2-3 ml. urine thus remain unmixed with alkali, which serves as a control of whether the urine has been sterilised by the temperatures to which it has been submitted. I repeat, this is always the case with urine of normal acidity.

Stand the vessel in the incubator.

Result: Organisms never form." (Oeuvres, Vol.2, pp.472-473).

**Fig. 15.**

Pasteur's apparatus for his experiments with urine.

With this, Pasteur concluded his side of the debate. What Bastian presented to the commission and whether and how he replied to Pasteur's experiment, I have not been able to discover. So I shall merely repeat, with due reservation, another remark of the previously mentioned Abbé Moigno:

> "But when the day, the hour, the choice of judges and the place had been set, when he had been informed of the programme of experiments and his request for the elimination of certain scientists, who were not sympathetic towards him and their replacement by others whose views he believed to be closer to his own, had been refused, Dr. Bastian suddenly departed for London..."

It seems to me entirely understandable that after this Pasteur and Tyndall were, for the time being, regarded as the incontestable victors in this struggle, at least in the experimental field.

## 6. Haeckel & Naegeli.

Surveying the conflict between experimentors on the question of spontaneous generation, as here outlined in its outstanding features, it is easy to see that, and why, it could not lead to any satisfactory ideas on biogenesis. None of the questions of the classification, the structures and structural laws, the physiology, or the biochemistry and biophysics of "simple life", inseparable from the main problems of these researches, had yet been worked out. A supporter of the doctrine of spontaneous generation, such as Bastian, was, naturally, very much aware of this - at the end of his Evolution and the Origins of Life he declared that the discovery and explanation of the genesis of life seemed to him to be "at present an almost hopeless" task, and expressed the opinion that it should be approached via investigation of the phenomena of grbwth (op. cit. p.196). With the development of cytology, biochemistry and biophysics in the latter quarter of last century, a new period in biology began in relation to our question. Since the sixties Abbe had very considerably improved the immersion objective. Preparation and staining technique had made great progress during the same period (Waldeyer introduced haematoxylin in 1863; O.Hertwig, borax-carmine in 1875). After the example of his and others, more general use was made of the microtome after the sixties. Between 1873 and 1876 Schneider, Bustschli, Hertwig and Strassburger discovered and described mitosis. The fusion of cell nuclei in fertilisation was definitely observed by the brothers Hertwig, and by Bustschli and Fol. (Omnis nucleus e nucleo!) Du Boys-Raymond's electro-physiology, Wilhelm

Pfeffer's osmosis experiments, and so on and so forth - all these advances in knowledge and experimental technique forced the theory of spontaneous generation into the realm of pure speculation, and soon afterwards it almost completely ceased to engage the active interest of biologists.

Theories of spontaneous generation, as they appeared for a time after the victory of the theory of evolution, can be divided into three groups: general "progressionistic" evolutionary theories: pan-animata, "organic cosmos" theories: and more specialised chemical and physical theories. But they are all better described simply as speculations.

Ernst Haeckel appeared on the scene first with his "Monera" theory. "The assumption of the genesis of Monera from inorganic carbon compounds" is demanded by "the reasonable causality-need of human understanding", as an "indispensible hypothesis", "a logical postulate of philosophical natural science". Only for this structureless "primal slime" ("plasson"), the "most ancient material bearer of a life-activity", can and must a process of spontaneous generation be postulated, but "not for the genesis of even the most simple cell".

Such structureless lumps of protoplasm have not, so far, been found. What Thomas Huxley found at the bottom of the sea, and believed to be "Bathybius haeckelii", turned out to be an inorganic colloidal substance consisting mainly of gypsum.

Haeckel conceived of the process of spontaneous generation as a kind of crystallisation. Furthermore, he came increasingly to the conclusion that even atoms were animate, and assumed a sort of elementary organism - "plastidules", "animated atom complexes". He developed this theory of pan-animacy still further in Welträtseln and in the work of his old age, Kristallseelen.

In his Mechanisch-physiologischen Theorie der Abstammungslehre, Carl Wilhelm Naegeli expressed his opinion in the same words as Haeckel: "To deny spontaneous generation means to proclaim miracles". For him also "the genesis of the organic from the inorganic ... is not, primarily, a question of procedure and experiment, but a fact which follows from the law of conservation of matter and energy". Spontaneous generation must continue to operate everywhere where conditions are similar to those of the primal age (when the surface of the earth had cooled to the temperature which permitted life). But even Haeckel's Monera were not simple enough for Naegeli. Living drops of homogenous, unstructured and undivided proto-

:plasm, consisting "merely of albuminates", probably developed from ammonium carbonate "in the moist upper layer of a finely porous substance (clay, sand) where the molecules forces of solid, fluid and gaseous phases operate in conjunction". From this the developmental process continued "on the one hand via urine or ammonium cyanate and then via nitrogenous compounds. such as asparagine, etc., and on the other hand via ammonium tartrate etc. to protein bodies", "as is also the case in the nutrition of fungi".

According to Naegeli, "this would imply both growth and propagation, and therefore spontaneous generation". New protein "micelles" would form between the existing ones and the growth of the protoplasmic mass would sooner or later cause it to disintegrate into two or more masses.

In time, even if very slowly, growth and propagation must cease to be uncontrolled or only the result of external circumstances, and become "internally regulated" processes. Only then would an organism proper have arisen.

The series of forms which Naegeli assumed between the "primordial protoplasmic mass" and the most simple organisms known to us, he called "protobia".

The disintegration of the "primordial protoplasmic mass" was still entirely dependent upon chance external causes, hence it grew to considerable but quite indefinite sizes. The internal organisation was only later able "to master division and overcome cohesion" - for this reason, and because they showed movement, Haeckel's Monera, which retained a definite size, were not lumps of "primordial protoplasm".

Spontaneous generation presupposes not simply the presence of a protein, but protein-formation.

> "For only if protein develops can the micelles enter a configuration appropriate to their molecular energies, and only by continued synthesis of protein can they preserve this character during growth."

Hence it is impossible:

> " ... to produce anything organised by artificial means. For all organisms are subject to micellar conditions, and molecular energies develop which are only present in the organism concerned and cannot be imitated.
>
> The life of a protoplasmic mass therefore consists in the fact that the specific configuration of its structure remains undisturbed, and it is certainly a matter here of the configuration of the idioplasmatic system alone."

The proposition that it is impossible to produce anything organised by artificial means, obviously implies that it will never be possible to generate (to cause to develop from the inorganic) living forms whose structure can be predicted from the experimental conditions. The path of experimental spontaneous generation must, therefore, lead to "protobia" via protein synthesis.

Like Bastian, Naegeli was also of the opinion that new species could proceed from the newly developed protobia:

"Spontaneous generation, and with it the commencement of ascending series, has always taken place, and still take place to-day, as it did in the beginning". (op. cit. p.15)

For Naegeli the diversity of living forms resulted from the "perfection principle" or "principle of progression" innate in the living, and from adaptation. The above principl represented for him "the law of persistence in the realm of organic development", according to which the living continuall advances towards "more complicated structure and greater division of labour".

7. Fechner & Preyer.

While Haeckel and Naegeli thus endeavoured to demonstrat both the necessity and the reasons, so far without experimenta confirmation, for assuming spontaneous generation, hypotheses were developed by others which were supposed to render the "continuity of life" consistent with the ever-changing conditions during the earth's history. H.E. Richter (1865), W. Thomson/Lord Kelvin (1871), H. Helmholtz (1876), von Bunge (1905) and finally Svante Arrhenius (1908) advocated a conception called by W. Preyer, who also favoured it for a time, the "cosmozoa hypothesis". According to this, some primitive life-form was once, or on several occasions, trans-:planted to the earth from the cosmos or from other planets in the shape of minute micro-organisms or pre-stages of living matter, either in the interior of large meteors or else (Arrhenius) by the pressure of light rays.

It is clear that the real significance of this hypothesi lies in the fact that it "prohibits" the question of abiogenes-:sis. This is the sense in which it has been generally under-:stood, and rejected as unsatisfactory.

G.Th. Fechner (Einige Ideen zur Schöpfungs- und Entwickelungs-geschichte der Organismen, 1873) and W. Preyer ("Die Hypothesen über den Ursprung des Lebens", in

ssenschaftliche Tatsachen und Probleme, 1880) rejected
otheses of both spontaneous generation and cosmozoa as
improbable. A reversal of the whole question seemed
to be more serviceable. From the beginning of time
as life and out of it, in the course of the development
earth (and the other planets), the inorganic world
i off.

echner saw the nature of life as "a condition of organi
y". The solar system shows "in the relationships of
nets a picture of the behaviour of the particles in an
molecule" (op. cit. p.3). But there exists in the
"tendency towards stability". The inorganic molecules
hemselves in a condition of mobility close to complete
ty. It follows from the above principle, therefore,
and large, development must proceed from organic to
ic, and not vice versa. Now the whole of primordial
was, according to Fechner, a unitary, vast, dark and
rimordial organism of "cosmorganic" constitution, in
organic movements" were generated by gravitation. The
also, including the earth, which had condensed out of
ial matter, showed cosmorganic life. With the progres-
ncentration of matter, there appeared a "transformati
cosmorganic movements in molecules", "which everywhere
through the molecular-organic state". But this was
preserved only on the surface of the earth of to-day,
he heat generated by condensation was sufficiently
ated by radiation and cooling. Firm land, primordial
nd atmosphere were covered or "permeated" by a dense
ve, organic vesicular froth", which then "differentiat
to organic and inorganic in the present-day sense.

or W. Preyer: "Life is ... movement of discrete parts",
made no speculations as to the nature of this movement.
ming into being of the movement-complex 'life'", is
up with a particular kind of arrangement of the parts
ch we do not yet know and which must always have been
i somewhere". It was not the "life-capable" that arose
e complex of conditions necessary to call into life an
tain alive precisely the contemporary forms of the
life of our earth that did arise, i.e. did not always

"Living and life-capable combinations of material
s have always existed somewhere in the cosmos, and have
d on everywhere where certain external conditions were
lised." (op. cit. pp.316-317)

"We do not say, therefore, that protoplasm has existed
uch since the beginning of the formation of the earth:

nor that, as such, it migrated without beginning from somewhere else in the universe to the cooled earth; still less that it constituted itself out of inorganic matter on the lifeless planet, as the theory of spontaneous generat-:ion has it. But we assert that the beginningless movemen in the cosmos is life, that protoplasm had of necessity to remain over, after the matter now described as inorganic had been expelled on to its cooling surface by the more intense life-activity of the molten planet, without being able to re-enter the continually decreasing quantity of hot fluid, on account of the progressive fall in tempera-:ture of the earth's crust.

... one could even seek, extending the conflict abou existence to all bodies, to prove that of necessity, on cooling, the elements of protoplasm were bound to survive in just this combination by virtue of their physical and chemical characteristics and their movement-states brought about in the heat during the course of millenia." (op. cit. pp.60-61)

L. Rhumbler remarked ("Anorganisch-organismische Grenzfragen des Lebens", in Driesch-Woltereck, Das Lebens-:problem, 1931), regarding the Fechner-Preyer hypotheses:

"These theories, which, on closer inspection, are no so abstruse ... only apparently avoid the question of spontaneous generation. They give an unjustifiably wide scope to the concept of life and do not reveal to us how plant and animal bodies are supposed to have condensed out of gases and fluids. But precisely this constructive condensation of the living out of other substances is the content of the question of spontaneous generation." (p.19

8. Pflueger, Allen & Oparin.

In 1875 E. Pflueger attempted, as Rhumbler expressed it a "much closer basing of spontaneous generation in the realm of the inorganic" in his essay "Über die physiologische Verbrennung in den lebendigen Organismen" (Pflügers Archiv fü die gesamte Physiologie, Vol.10) by the "cyanide hypothesis". Living, metabolising protein is distinguished from dead prote by the possession of the cyanide radical (CN). In conjunctio with oxygenation, CN gives living protein its high capacity for auto-catalysis.

"In considering the beginning of organic life, one must not think primarily of carbon dioxide and ammonia. for both are the end-products of life, not the beginning.

... The beginning consists rather in the cyanide radical."

Now CN, like hydrocarbons, the alcohol radical and other components of protein, is formed synthetically at red-heat.

> "We see how quite extraordinarily and remarkably all the facts of chemistry point to heat as the energy which generated the constituents of protein by synthesis. Life, therefore, derives from heat and, in its basic preconditions, originated at a time when the earth was still a molten mass ... If we now consider the immeasurable ages occupied by the infinitely slow cooling of the earth's surface, cyanogen and the cyanide- and hydrocarbon-containing compounds had every opportunity to follow to the full their strong tendency towards transposition and polymerisation and, together with oxygen and later water and salt, to change into that auto-catalytic protein which is living matter."

In 1899 F.J. Allen came forward with a similar chemico-physical hypothesis ("What is Life?", in Proceedings of the Birmingham Nat. Hist. & Philos. Society, Vol.II, Part 1). For him, however, as for Haeckel, the presence of liquid water was an indispensable precondition for the genesis of life. Of the essential chemical elements of living matter (nitrogen, carbon, hydrogen, oxygen, sulphur and iron), nitrogen is above all the best adapted to spontaneous generation by the fact that its chemical compounds are very active and, in consequence easily broken down, as is demanded by organic metabolism. Oxygen probably played an important role in this, by the fact that it is continuously united chemically to the nitrogen, to be torn away from the latter again on the formation of water and carbon dioxide through its greater affinity for hydrogen and carbon, which is bound up with a high production of energy. At the time when water had condensed on the earth's surface, the heavier, more stable, insoluble compounds had sunk to the ground, and the more labile and more easily decomposed compounds were partly contained in the air as gases and partly in the water in solution. Flashes of lightning shot through the moist, warm, electrically highly charged atmosphere, and through these discharges arose ammonia and oxides of nitrogen, as still happens on a smaller scale to-day, which were dissolved and brought down by the rain. There was an abundance of carbonic acid in the water, and the chlorides, sulphates and phosphates of the alkali and heavy metals were also present in solution in the water. Thus, under favourable conditions, the elements interacted upon one another so as to combine into the first living substance, essentially similar to that of to-day. Allen does not enter into details, but merely mentions as a possibility that iron might have functioned as an oxygen carrier (as in haemoglobin).

According to E.O. von Lippmann, the Stuttgart zoologist Jaeger was supposed to have advanced the theory that living matter developed exactly like a crystal from appropriate solutions, and that this took place "under the influence of electrical tensions between different chemicals". I have not so far been able to learn more about this view.

Hypotheses similar to those of Pflueger and Allen have been advanced by Zehnder (1899), Verworn (1903), who called the "living protein bodies" biogens, B. Weiss (1907), P.Jensen (1907) and, quite recently, A.I. Oparin (1936). I shall confine myself to repeating here part of Joh. Dembowski's review of Oparin's book The Origin of Life on the Earth (Russian) in Berichte über die wissenschaftlich Biologie, Vol.41, 1937, p.145:

"Only in the process of the long development of a primary organic substance could even the simplest living organism be formed on the earth... In the capacity of the, in itself not organic, carbon to form long chains of atoms and to form compounds with other organogenetic elements, lies the possibility of forming, under certain conditions, the beginning of living matter. Spectroscopic and geo-chemical researches prove that carbon could not make its appearance on the earth's surface in the form of free carbo dioxide, but of hydrocarbons; and in view of the temperat-:ures characteristic of that period of the earth's history, first of all in the form of free radicals which combined to form unsaturated hydrocarbons of the ethylene and acetylene groups. These circumstances are sufficiently well known to us through the Krack process. The hydrocarbons must have been oxidised at the expense of the oxygen bound up in the water. Together with ammonia, amine and amides were formed so that in the earth's primary hydrosphere there existed solutions of organic substances whose molecules contained hydrogen, oxygen and nitrogen besides carbon. Character-:istic of these compounds were the enormous chemical possibilities resulting from the properties of the atoms forming them and their arrangement in the molecules. Hence the most varied chemical reactions, oxidations, reductions, condensations and polymerisations could take place between them in the solutions of these organic compounds. These processes led to the formation of large, complex molecules, similar to those of which plant and animal organisms are built up to-day. In this way the biologically most important compounds, the proteins, were formed.

In the mixture of the colloidal solutions of protein compounds arose particular structures, the coazervates (de Jong, 1932). In semi-fluid colloids, organic compounds concentrated at certain points, formed more or less sharply

defined margins between themselves and the solution medium, and developed into elementary structures. Thus individual formations arose, which, because of their specific physico-chemical structures, were able to adsorb and metabolise organic compounds from the environment. A modification in the internal structure of the coazervates went hand in hand with the resulting growth, and only those formations were capable of continued existence in which this modification was of a progressive character. Thus a particular kind of natural selection made its appearance, which finally led to the genesis of systems containing enzymes which evolved a mutual coordination of biochemical processes. In this way arose the first primitive organisms.

The oldest form of nutrition of the organism was not autotrophic but heterotrophic. As regards energy metabol-:ism, the butyric acid type of fermentation represented a primary and still imperfect stage. At later stages of development, lactic acid and alcoholic fermentation appeared as more regulated processes. But for a long span of time all metabolism was based on anaerobic fermentation alone, since at this period the atmosphere of the earth still contained no oxygen. It was only formed much later as a result of photochemical reactions. To begin with, these merely made possible a more efficient utilisation of existing organic compounds and was not the basis for autotrophic nutrition. Only later did systems arise operating with this kind of nutrition on the basis of photo or chemo-synthesis. The capacity for respiration only appeared when the air contained a considerable quantity of oxygen. The inner chemical mechanism which permitted full utilisation of oxygen for metabolism was only an additional perfection of the primary anaerobic metabolism...

The gigantic task of the future consists in discover-:ing the individual steps in this developmental process. But this requires as preliminaries, investigation of the protein molecule, of the structure of colloidal organic systems, the study of enzyme systems, protoplasm structures etc. This difficult and lengthy path will lead us to the comprehension of life."

9. Contributions from crystallography, colloidal and physical chemistry, etc.

Two features common to all these hypotheses - other than those which assumed the "eternity of protoplasmic life" - seem to me most worthy of note. For a time they all dealt exclusively with "archebiosis" (in Bastian's sense). Then they all strove to find plausible explanations of the mechanism of the process, the assumption of which appears irrefutable, by which (protoplasmic) "life" once proceeded from "non-life"; and also for a generally applicable conception of the dynamic of life-phenomena and its integration into the dynamic of natural processes as a whole. Under the fresh influence of the victory of the theory of evolution these hypotheses were advanced boldly and confidently - above all by Haeckel. But with the ever-mounting mass of the factual data of biology, the prospect of an immediate, reasonably satisfactory hypothesis regarding biogenesis was bound to become increasingly remote. The following passage of L. Brüel (from the article "Zelle und Zellteilung, zoologisch" in Handwörterbuch der Naturwissenschaften, 2nd edition, vol.10, Jena, 193[illegible] seems to me typical of the position of the problem:

> "Metabolism maintains life; chemical changes provide the cell with energy which is used up in movement, etc. We are confronted with a chemical system whose equilibrium is easily and continually disturbed. The order, the determinate but irregular direction of this process has always been the most mysterious factor..." (my italics, A.H.

It is therefore not to be wondered at that the gigantic mass of material collected by strictly empirical and non-metaphysical individual researchers should have contributed to a resuscitation of the metaphysical trains of thought of vitalism, which appeared to have been overcome in "neo-vitalism" (von Rindfleisch, 1888), on this question of the "order", "direction", "goal" and "plan" of the living.

This is not the place to enter into the extra-scientific bases of vitalistic attitudes. (The vitalist, von Uexkuell, said, "In the last resort, even the individual researcher's conception of the organism depends upon his world-view".) I shall only suggest here how, in the continued absence of a successful and unchallenged experiment in spontaneous generation, the question of the genesis of life was slowly forced out of theoretical biology by the renewed conflict over fundamentals. Rudolf Ehrenberg, for example, who attempted to avoid vitalistic trains of thought by attributing the "directed course" of the life processes to a "law of the life-necessity of death", which was "a biological analogy to the law of entropy", remarked on our problem:

"A theory of development with spontaneous generation at the beginning and structural metamorphosis brought abou by crossing, mutation and selection in its continuation, starts with an extreme improbability in order to establish that everything beyond that follows statistical laws. Thi is an impossible position. The conclusion can only be tha the genesis of life on earth is a problem which biology cannot solve. The well-known meteorite theory, the idea o infection from the cosmos by tiny life-germs, achieves, by postponing the problem, all that can be achieved." (Theoretischen Biologie vom Standpunkt der Irreversibilitä des elementaren Lebensvorganges, Berlin, 1923, p.271)

But the same development which, with regard to the speculative solution of the problem of spontaneous generation led to resignation, naturally opened up new avenues, on the other hand, by bringing about a progressive dissolution of conceptions of a sharply defined boundary between inorganic and organic. Thus L. Brüel continued, in the article mentioned:

"We have now learnt - for the moment theoretically - how truly gigantic forces, e.g. of an osmotic and electric nature, reside in the surface phenomena of dimensionally small many-phased systems and how, by such means, the chemical processes and all the alterations of state derive from these sources of energy can be ordered in space and time. Chemical organisation seems to us possible and explains an astonishing number of vital phenomena." (Italics in original)

Endeavours "to reach a solution of the question of life and of spontaneous generation on the basis of analogies of physical and chemical phenomena" (von Lippmann) were founded, above all, upon the findings of crystallography, colloid chemistry and certain structural-chemical investigations related to Pasteur's researches on molecular asymetry. The study of diffusion and the separation phenomena of immiscible fluids brought many surprising analogies to light. Quincke, Buetschli and Rhumbler, in particular, occupied themselves with the "Methodik der Nachahmung von Lebensvorgängen durch physikalische Konstellatione" (methods for the imitation of life processes by physical configurations). (Vide, especiall the last section under this title in Abderhalden's Handbuch der biologischen Arbeitsmethoden, Section 5, Part 3A, 1921, pp.219-440; as well as his definitive review "Aus dem Lückengebiet zwischen organismischer und anorganismischer Materie" in Ergebnisse der Anatomie und Entwicklungsgeschicht Vol.15, 1906, pp.1-38).

I shall enumerate some particularly interesting experiments of this kind, mostly taken from Rhumbler's exposition.

J. Butler-Burke ("On the Spontaneous Action of Radio-active Bodies on Gelatine Media", in Nature, Vol.72, No.1856, 25th May 1905) sterilised gelatine and, with the usual sterile precautions, placed 2½ mgm. radium bromide on the surface of the gelatine. After 24 hours in some cases, and 3-4 days in others, a curious culture of spheroid formations, which divided after reaching a diameter of about 0.0003 mm. appeared on the surface of the gelatine. Occasionally they were accompanied by rosette-like formations. Control dishes without radium showed no "radiobes" of any kind.

When the culture was heated the bacteria-like forms vanished, but could again be observed under the microscope after some days. They dissolved in warm water. The transfer of such cultures to fresh media was unsuccessful. After a month the quantity of the inoculated culture was negligible, and certainly too small to be a culture of any bacteria.

Rudge (Proceedings of the Cambridge Philosophical Society, 1906) then succeeded in producing similar growth phenomena on gelatine with barium, calcium and strontium salts He gave the following explanation. The causes of the apparent cell formation are sulphuric and sulphurous acids and their salts, in which strongly bleached gelatine is rich (bleaching was done with sulphurous acid). They precipitate insoluble compounds from solutions of salts of radium, barium, strontium or calcium, the individual particles of which represent the "nuclei" of the artificial cells; while the cells themselves consist of small vacuoles, formed when the insoluble compounds precipitate out. If gelatine is carefully freed from sulphuric acid and sulphates, prolonged contact with radium salts produces no radiobes, which do, however, develop immediately the slightest trace of salts of sulphuric acid, of which ordinary water contains sufficient, is added. Radiobes, which, of course, resemble organic cells only in their outer form, can be obtained from rubber, and other gelatinous materials, with ordinary water.

I have not been able to obtain further details regarding Martin Kuckuck's "barium cytodes" or Raphael Dubois' "ecbes". The names suggest that they refer to phenomena similar to those observed by Butler-Burke. I found the following definition in W. Guttmann's Medizinische Terminologie, 10th and 11th editions, 1919:

> "Microbioids (R. Dubois) - Bacteria-like organic-mineral formations, which develop in gelatine on the addition of radium chloride."

In drops of a mixture of equal quantities of 33% sodium chloride solution and ammoniated 90% alcohol, which had stood for ½ hour in beakers under a glass cover, Littlefield observed not only salt crystals but also crystals from which issued small oval or spherical formations, which he held to be living since they exhibited growth and amoeboid movements. (Vide, Rhumbler, 1906).

W. Roux reported his controls of Littlefield's experiments in Umschau, Jahrg., 1906, No.8. According to him, it is a matter of separation phenomena, the separation of crystals on the microscope slide producing currents and movements, due to the unequal surface tensions of the substances present. What Littlefield thought were living organisms, seemed to Roux to be "residues or precipitates of substances left behind on unclean parts of the slide on drying". Drops of condensing alcohol vapour on the sides of vessels exhibited similar phenomena.

A. Wieler's "sprouting fungus formations", which he reported in Berichten der Deutschen Botanischen Gesellschaft, Vol.22, 1904, pp.541-544, "have not," according to Rhumbler, "received a satisfactory physico-chemical explanation". He made microscopic examinations of 90 precipitates of the salts of Ca, Ba, Mg, Al, Zn, Cd, Be, Ag, Cu, Pb, Fe, Co, Ni and Mn, precipitated by potassium carbonate, sodium carbonate, ammonium carbonate, sodium bicarbonate, potassium acid phosphate, potassium acetate, potassium iodate, borax, oxalic acid, sulphuric acid, hydrochloric acid, potassium hydroxide and ammonium hydroxide. The practical value of these and similar observations lies primarily in the manifold incentives they gave to cell physics. The same applies to precipitation membranes investigated particularly by G. Quincke and Stefan Leduc by the Moritz Traube process - "metallic salt vegetations" and so-called "artificial cells". (The semi-permeable membranes with which W. Pfeffer and van t'Hoff carried out their osmotic pressure investigations were first produced by Traube's experiments.)

Of the "artificial vegetations", known principally through Leduc, Paul Kammerer (Allgemeine Biologie, 2nd edition, Stuttgart/Berlin, 1920) said:

> "The ... hypothesis regarding genesis of life on the earth has recently received great support from physical processes. Osmotic pressure .., in the experiments of Leduc, Quincke, Benedikt and Stadelmann, enables inorganic substances to develop into formations, resembling branched fungal hyphae, which appear deceptively like certain group and species of living organisms. If, for example, the bottom of a crystallising dish is covered with pure sand,

strewn with various sized crystals of potassium chromate and iron and copper sulphate, and if the dish is then filled with a dilute solution of water glass and left undisturbed, what appears to be a vegetable growth consist:ing of blue, green and brown branches develops. Partic:ularly striking is the fact that when the osmotic format:ions develop in fresh water they actually resemble forms which are to be found in inland waters, filamentous algae, mould fungi, mosses, fresh-water shells, etc; but when the grow in sea water they resemble marine forms, such as tube worms, sea-snails, oysters, Hydrozoa, Actinia, corals, etc The osmotic formations have much in common with genuine living organisms, not only in their outer appearance, but also in their internal, cellular structure, in the mode of metabolism and growth. Leduc in particular drew sweeping conclusions regarding the genesis of life from his product:ions, believing that the spontaneous generation, not merely of unicellular organisms, but even of higher plants and animals up to the vertebrates, had become evident through his procedure." (p.24)

Leduc also drew attention to the extraordinarily cell-like diffusion formations obtained by introducing into one solution drops of another, not isotonic, solution.

Otto Lehmann has, since 1889, devoted himself especiall to the investigation of the phenomena accompanying what he called "fluid crystals", discovered in 1888 by Friedrich Reinitzer. According to R. Brauns in Handwörterbuch der Naturwissenschaften, 2nd edition, Vol.5, 1935, p.1159 et seq:

"Fluid crystals are homogeneous, chemically pure compounds which, within certain temperature limits, combin fluid characteristics (e.g. the lack of an elasticity limi with the characteristics of a crystal, with double refract:ion. In view of these characteristics, these bodies have been variously called anisotropic fluids, crystalline flui double-refracting fluids, fluid crystals, flowing crystals viscous crystals, and a certain group, in view of peculiar movement phenomena, apparently-living crystals. G. and E. Friedel called the last-named substances mesomorphous, and distinguished a "smectic" and a "nematic" phase, which would correspond to Lehmann's flowing and fluid crystals. D. Vorländer, however, regarded these expressions as entir:ly mistaken. Fr. Rinne called the fluid crystals para-crystalline, i.e. they have only two dimensional symmetry, instead of three dimensional."

There still exist to-day, therefore, wide differences o opinion as to the nature and definition of fluid crystals. The comparison with organisms, which , together with an expan

:ion of the conception of life, was gradually pressed by O. Lehmann as far as equation, seems to have been more or less unanimously abandoned by researchers - if not by natural philosophers. And it is not mentioned at all in the discuss-:ion on fluid crystals, the "mesomorphous state" and especially "eumesomorphous" substances (Wo. Ostwald), in No.1-4 of the Zeitschrift für Kristallographie, Part A, Vol.7 1931.

Otto Lehmann described as "apparently living" the flowing crystals of ethyl-p-azoxycinnamate (manufactured by E. Merck of Darmstadt), first observed by D. Vorländer. They appear (Vide, Rhumbler, 1906) when this substance:

> "... is moistened with a trace of solution medium (1-bromo-naphthalene is suitable) and heated almost to a molten state under the microscope. The melting point must not be exceeded, since if it is some degree of disintegrat-:ion sets in. The drops, which are of about the consisten-:cy of olive oil, are not absolutely spherical. One one spot they show a funnel-shaped depression, from the centre of which a straight line leads to the centre of the drop. From this opening may suddenly emerge a second drop, attached to the parent-drop by a cylindrical tube, from which it become detached when it has attained the size of the parent sphere. But separation is comparatively rare. More often the bud grows into a long, worm-like form whic: twists and bends..."

Besides these vermiform and serpentine formations, otners are to be seen which look deceptively like Infusoria, Amoebae, bacteria and sperms. Division, similar to that of bacteria, can be observed. According to Lehmann:

> "A rod can suddenly break in the middle, after a kind of septum has spontaneously developed at this point, evidenced by a peculiar refraction resulting from twin-formation."

Growth takes place through interposition - not, as in solid crystals, through deposition - hence, according to Lehmann, producing movement phenomena.

Otto Lehmann himself, in the course of his investigat-:ions, approached closer and closer to Haeckel's belief in "crystal souls", while D. Vorländer, who next to him was the most-occupied with these researches, rejected such comparison as unscientific.

Otto von Schroen developed his views on "bioplasm" fro

the study of crystal formation in hanging drops of saturated crystalloid solutions. The phenomena of growth and division which appeared during this process he interpreted as life phenomena. According to him, "bioplasm", the universal life former, appeared in a series of modifications: "protobioplasm" generated planets, "petroplasm" crystals, "phytoplasm" plants "zooplasm" animals, "anthropoplasm" mankind, etc. (Vide, Rhumbler, 1906)

The numerous observations and interpretations of this kind doubtless greatly contributed to the fact that, despite the understandable disposition of specialist researchers to draw rigid boundaries between their respective fields, they could never quite exclude the study of this "borderline question" from science. It can be said that here, as in other fields, real progress ultimately derives from the continual refutation of premature generalisations and exaggerated, dubious "final decisions". The struggle which Lehmann, Vorländer and Schenk had to carry on against van t'Hoff, Quincke, Tammann and Nernst for the recognition of fluid crystals is characteristic of this. (But in 1928 Albert Mary numbers Nernst amongst those who "acquainted us with fluid crystals".)

The work of O. von Schroen was closely paralleled by, e.g. M. Benedikt (Kristallisation und Morphogenesis; biomechanische Studie, 1904), Max Muenden (Der Chronoblast, die lebende biologische und morphologische Grundlage alles sogenannten Belebten und Unbelebten, 1906), Melan Janorski (Le Géon, ou la terre vivante, 1928), Albert Mary (La vie merveilleuse des mineraux), and others.

Among those who proceeded more carefully by confining themselves to analogies, I should like to mention A. Dastre (La vie et la mort, 1916, particularly book 4, La vie de la matiere), Hans Przibram (see particularly his Experimental-zoologie, Vol.4; Vitalität, 1913; and Die anorganischen Grenzgebiete der Biologie, insbesondere der Kristallvergleich 1926), Ludwig Rhumbler (apart from the works already named, "Das Protoplasma als physikalisches System" in Ergebnisse der Physiologie, 14, 1914)and Friedrich Rinne ("Grenzfragen des Lebens", 1913; and "Beiträge zur biologischen Kristallographie four articles in the Centralblatt für Mineralogie, ~~Jahrg~~, 19 Part A).

Friedrich Rinne, supported by W.I. Schmidt's investigations on living and preserved spermatophores of Sepia officinalis (cuttlefish) and his own parallel researches, advanced the opinion that the chromatin granules of the sperm nucleus (spermatomerites) must be included amongst the fluid crystals. For they exhibit:

"1. A consistency such as is found in artificial fluid crystals.

2. Double refraction, which is not caused by secondary or external, but by internal conditions, and is the same in all samples of the substance." (Cbl. f. Min., 1931, A, p.233-4).

In Rinne's opinion sperms represented:

" ... independent living organisms ... which have at the same time properties of colloidal gels and of crystals ... a link between inorganic and organic nature which has functioned since primordial ages." (Grenzfragen des Lebens 1931, p.63).

The opposite conception, which R. Brauns championed in the Neuen Jahrbuch für Mineralogie, 1931, Review section, 1,1, was maintained in the aforementioned article in the Handwörterbuch der Naturwissenschaften. His main argument ran "Sperms are extremely inhomogeneous bodies, fluid crystals are homogeneous". And he drew support from a proposition of Ar. Johnsen:

"Without inhomogeneity no life! Inhomogeneity is a fundamental characteristic of living matter, as opposed to minerals or crystals." (über den Unterschied von Mineralie und Lebewesen, 1930).

Rinne's demonstration (Cbl. f. Min., p.238) "that a further complication, namely, that molecular mixture which is found in living matter, is by no means unknown in fluid-crystalline substances", was completely disregarded by Brauns.

It may be of interest in this connection, that the French biologist le Dantec disputed the classification of spermatozoa (and ova) as forms of life. He said:

"Mature sexual substances, spermatozoa and ova, are dead. They are incapable of assimilation under the conditions favourable to their species, they are incomplete." (La Lutte Universelle, 1920, p.228).

But since the capacity of very many ova for parthenogenetic development has to-day been proved, this viewpoint may only apply to some extent to sperms.

Attempts to reach an explanation of the origin of life by means of colloid research are not known to me. In fact, it is precisely colloid chemistry which has most powerfully supported resignation with regard to this problem, by revealin

enormous, hitherto entirely unknown complications in the structure and dynamics of "living matter".

On the other hand, the asymmetry of protoplasm, first discovered by Pasteur in 1860, made possible certain precise suppositions on our problem.

Felix le Dantec reported (op. cit.) that Pasteur first held molecular asymmetry to be a phenomenon peculiar to living matter, but when, soon afterwards, a compound with such a molecular structure was synthesised, he retreated to the assertion that it would only be possible to produce mixtures containing equal quantities of both optical isomers. In 1913 Japp wrote, on this question:

> "The appearance of any asymmetrical compound without the inverse optical isomer is completely inconceivable, without the intervention of a previously existing asymmetr-:ical force. Now, apart from those of life, there are no asymmetrical forces on the earth, consequently asymmetry could not have appeared on the earth prior to life. Rather life itself, which is manifested in asymmetrical compounds, could only appear under the influence of a pre-existent asymmetrical force, an intelligence capable of choosing between two asymmetrical compounds. From this follows the necessity of a creation." (Vide, le Dantec, op. cit., pp.242-3).

Amongst the many arguments against Japp's views, le Dantec raised that of Herbert Spencer, that the rotation of the earth is itself such an asymmetrical force. Pasteur also began to give consideration to this suggestion, but did not proceed further. le Dantec himself remarked that their simultaneous formation does not necessarily imply that the two optical isomers remain together. The development of life, therefore, might very well have proceeded from a suitable compound of the laevo-rotatory series, to which the basic chemical constituents of protoplasm belong.

G.F. Gause summarised the most important facts and theories relating to this question in a survey, "Raumaufbau des Protoplasmas" in Ergebnisse der Biologie, Vol.13, 1936, pp.54-92. The following are some quotations:

> "The asymmetrical characteristics of (protoplasmic) matter are just as necessary to organisation as the colloidal structure, but merely at a different level." (p.59)

> "As Pasteur assumed, and E. Fischer later clearly formulated (1898), plants and animals consist of asymmetr-

:ical compounds because the synthesis of organic matter in protoplasm proceeds under the influence of asymmetrical catalysts. The transformation of carbon dioxide into sugar clearly takes place in the plant with the aid of the optically active substance in the chloroplast. Such an asymmetrical synthesis, in Fischer's sense, was soon realised in the laboratory of Marckwald and Mc'Kenzie in Bälde, 1904.

Curie's instructive proposition (1894): 'If any phenomena exhibit asymmetry, the same asymmetry must be present in the causes which produce these phenomena', explains why the asymmetry of protoplasm, once having arisen, is maintained. The question of the 'primary asymmetrical synthesis', i.e. the conditions of the first appearance of asymmetrical catalysts on the earth, however, remains completely open. A certain attempt at clarifying this problem was made by Byk (1904). He endeavoured to prove that rotation of the angle of polarisation of light could be produced on the earth by extra-terrestrial causes. It is known that sunlight contains a certain amount of plane polarised light, its angle depending upon the positic of the sun. If it falls on the surface of the ocean, its angle of polarisation is rotated on reflection. Under the influence of terrestrial magnetism, either the left or right-rotated light will predominate. In other words, equa amounts of oppositely rotated light cannot arise at the same time at a given point on the earth's surface. At the same time, Byk arrived at the supposition (exact proof was first provided by Kuhn in 1929) that, under the influence of rotated polarised light, a raceme (an optically inactive form consisting of equal amounts of both isomers) becomes optically active. This is due to the greater destruction of that isomer which possesses the greater power of absorbing the particular rotated light ...

It remains to be proved that the intensity of the cosmic process is sufficient to produce optical activity in racemic compounds. The fact that the main constituents of the protoplasm of all organisms are laevo-rotatory also remains entirely unexplained ...

It is still too early for us to make a decision on the question of the genesis of asymmetrical protoplasm, since we know nothing of the role played by asymmetry in the organisation of protoplasm as a whole." (pp.58-59).

I shall confine myself to these quotations, since the rest of the conclusions in this article, though not less interesting, are rather far removed from our present problem.

10. Evolution, ultra-microscopic bodies.

Felix le Dantec wrote:

> "What most astounds us in the ingenuousness with which Pasteur's opponents, the spontaneparists, accept the normality of the appearance of Infusoria in fluids, is that they are not surprised to find that what appeared were always animals or plants belonging to known species. And they were successors of Darwin!" (op. cit. p.239)

"Successors of Darwin" - that is, there had been a change in the hypotheses of spontaneous generation since Haeckel, insofar as it was generally assumed that the hypothetical "primal living organisms" must have been far "simpler" than all known organisms. From these, bacteria (and often also Rhizopoda) were supposed to have first evolved, or to be still evolving to-day. (E. Gotschlich in Vol.1 of the Handbuch der pathogenen Mikroorganismen, 3rd edition, 1929, p.36, draws a genealogical tree according to which Eubacteriales and Algae first evolved from a common hypothetical primal form. Connections then run from Algae and Bacillaceae, via Spirillaceae and Spirochaetales, to Protozoa; and from Bacteriaceae via Trichomycetes and Phycomycetes to Basidiomycetes. Ultramicroscopic viruses are related in the first place to Spirochaetales and Protozoa, and partly also to Bacteriaceae.)

After Protozoa had been rejected on account of their heterotrophic mode of nutrition (but see Oparin above), it was obvious to assume as the probable first forms of life, those bacteria which can exist independently of organic nutriment.

Carl Mez has now ascribed this rôle to the sulphur bacteria. Since I have unfortunately been unable to obtain his "Drei Vorträge über die Stammgeschichte der Pflanzenwelt" (Naturwissenschaft und Landwirtschaft, No.4. 1925) I shall sketch here his train of thought as reported by L. Brauner (Die Pflanze, 1930, pp.10-12) and W. Bavendamm ("Die Physiologie der schwefelspeichernden und schwefelfreien Purpurbakterien", in Ergebnisse der Biologie, 13, 1936, p.1, et seq.).

The first living organism must have been relatively insensitive to high temperatures, which would, however, have been below 100°C. Sunlight cannot have been essential to its life because of the continual dense cloud. Finally, it must have been capable of existing exclusively on inorganic matter. It is easy to imagine that sulphur, which might have become separated from a volcanic mass, say, in a colloidal state, combined with the co-existent compounds, water, carbon dioxid

and ammonia, to form certain simple organic compounds resembling the preliminary stages of protein. This would refute and replace Naegeli's theory of the first stage of life. To this Brauner remarked:

> "Attractive as Mez's line of argument is, it too leaves the great riddle of the animation of matter unsolved."

Bavendamm, who expressed no opinion on the question of spontaneous generation, said:

> "In this group of lower plants there may have developed a transition from the originally necessary chemosynthetic to the photo-synthetic mode of life, or a linking up of both processes."

Victor Franz, in Probiologie und Organisationsstufen, 1920, advanced the hypothesis that simple multicellular organisms first evolved from the non-cellular "archiplasm". Unicellular forms then evolved from these simple multicellular organisms by "reduction":

> "... as has been assumed by many botanists for Saccharomycetes, by Arthur Meyer for Eubacteriales, by Pascher for Pleurococcus, and by those I have quoted for Protozoa." (p.6)

This multicellular primordial living organism, in which cell nuclei might have arisen through "condensation" or by "the entry of smaller organisms into larger", might have developed the capacity for mitosis and for sexual propagation, and have thus emerged victorious from the struggle for existence.

The presence of "ultravisible probionts" in filterable viruses has also been supposed; or the conclusion has been reached through them, as also through d'Herelle's bacteriophagi, that there exist micro-organisms whose size is beneath the resolution limit of the microscope. Similarly, in filtered water:

> " ... organic substances containing carbon and nitrogen can be shown to exist, which do not seem bound to visible plankton." (Rhumbler, 1931 and 1935)

H. Miehe failed to provide proof by obtaining cultures of such ultra-microbes. ("Sind ultramikroskopische Organismen in der Natur verbreitet?", Biologisches Zentralblatt, 43, 192 pp.1-15). But as early as 1926 Bechhold and Villa were able to demonstrate formations 4-10 millimicrons in diameter in

bacteriophagi (Vide, Rhumbler, 1931; and H. Bechhold, Die Kolloide in Biologie und Medezin, 5th edition, 1929). L. Rabinowitsch-Kempner was able, in 1928, to prove with some degree of certainty, the presence of invisible filterable forms in the course of the development of the tubercle bacillus (Mycobacterium tuberculosis), as had already been asserted for some time by French doctors (Vide, L. von Bertalanffy, Theoretische Biologie, 1, 1932, p.238). According to a communication by W. Henneberg, reported by Rhumbler (1931, p.22, Addendum 16), the same has been successfully demonstrated for Bacillus tumefaciens, B.mycoides and B.coli. Finally, the Frankfurter Zeitung of 25.9.1936 reports from the 94th Congress of German Natural Scientists and Physicians in Dresden, that K. Herzberg and O. Waldmann had stained with victoria-blue and recognised as the excitants, the "elementary bodies" which had previously been observed in virus diseases. They are supposed to be 1 to 500 millimicrons in size. Virus cultures only subsist on living cells, preferably on the embryo of an egg.

Bechholt (Vide, Rinne, Grenzfragen des Lebens, p.45) expressed the opinion that:

> "Such minute organisms might be looked upon as transitional links between living organisms proper and inorganic matter, especially enzymes."

Even to-day, after 20 years, no clarification has been reached in the question of whether bacteriophagy in general is a matter of the activity of living organisms.

11. The present position.

Thus opinions are to-day still at variance as to whether spontaneous generation ever took place, whether it can still take place to-day, and whether it may be brought about artificially. Most researchers, insofar as they enter upon the question at all, believe with Rhumbler that the way to the synthesis of living protein will perhaps be found one day, with the amino acids as starting point, and that "with correct chemistry, the correct physics will very probably arise by itself" (Rhumbler, 1931, p.27). Hans Driesch, of course, said

> "Such assumptions are impossible to vitalistic, i.e. critical, biology. The possibility of such accidentally produced carbon compounds having provided the basis for the intervention of entelechy, which had been waiting for their existence, might be admitted. But even this assumption, quite apart from its phantastic character, is really

inadmissable because experience teaches that carbon compounds of a reasonably integrated nature never arise by chance, but are either the outcome of organic life-processe in the narrower sense, or else the result of the organising actions of a particular living being, namely the chemist .. In matters of spontaneous generation we can, therefore, simply say nothing at all." ("Das Wesen des Organmus", in Driesch-Volterech, Das Lebensproblem, p.448)

Finally, L. von Bertalanffy, who called his position "organic biology", which he claimed put an end to both mechanism and vitalism, even endeavoured to establish a "proposition of the impossibility of spontaneous generation". Max Hartmann (Philosophie der Naturwissenschaften, 1937, p.40) described this viewpoint as "in reality, merely a watered-down vitalism", and Philipp Frank (Das Kausalgesetz und sein Grenzen, 1932, p.115) as one of the "attempts to formulate vitalism 'positivistically'". He based his view upon "considerations of probability"- like Ehrenberg (see above) - and upon certain investigations by N. von Rashevsky (Zeitschrift für Physik, 59, 1930, p.558) who proved the impossibility of the spontaneous generation of a homogeneous drop, in a solution containing all the materials necessary to its development. Just as the primary thesis of physics is sometimes called the proposition of the impossibility of perpetual motion, so, analogously, said Bertalanffy, the "proposition of the maintainance of the organic system" might be formulated in the form quoted, "thus uniting in a truly instructive manner the two main problems of pre-scientific alchemy, Perpetuum mobile and Homunculus" (Theoretische Biologie, p.132; see also his "Studien über theoretische Biologie", in Biologisches Zentralblatt; 47, 1927)

This opinion of Bertalanffy's seems to me, however, to bear very little relation to the rest of his work. Even the natural philosopher Bernhard Bavink, who seemed to be in general agreement with Bertalanffy's other conceptions, did not believe that "the assumption of (mechanistic) spontaneous generation ... can be dismissed out of hand as impossible". (Ergebnisse und Probleme der Naturwissenschaften, 4th edition, 1930, p.302) And Philipp Frank declared the "probability" of the genesis of organisms by chance to be "entirely undefined":

"Since we know nothing whatever of a cycle of processes in the known or hypothetically assumed cosmos, there is not the slightest criterion for a judgement on the probability of the formation of an organic substance. The assertion that its development cannot be explained on account of its improbability is absolutely untenable, since probability or improbability can only be spoken of at all when the possibility of genesis has itself been admitted." (op. cit., pp.224-5)

Any suggestion as to the lines along which experiment, which in the last resort is decisive, might reach a decision and approach the solution of our problem, is outside the scope of this work. The disposition to exclaim "Impossible!" in this matter has already been ridiculed by Ernst Mach (1905) with the words:

> "If we were unable to extinguish fire, or to generate it, and were entirely dependent upon the use of that which occurred naturally, we should have every reason to say: 'Fire can only spring from fire'. To-day we know better." (Erkenntnis und Irrtum, p.297)

And Friedrick Kinne concluded his Grenzfragen des Lebens (p.122) with the remark:

> "It may very well happen tomorrow that an artificial protein is produced which possesses a particular energy content or is catalytically raised to the level of energy required, sufficient for the universally accepted biological activity of the lower kinds.
>
> Whoever forswears such events risks acting like the Academy of Sciences in Paris, which unanimously reached the conclusion that meteorites could not fall to earth from heavenly space, and to its astonishment underwent the experience that, in spite of this unanimously established "physical impossibility", the sky rained down a bombardment of stones on the earth."

APPENDIX. Remarks and addenda.

For Part 1, pp. , Aristotle to Pasteur, I have not used any original works, but have kept throughout to Pasteur's exposition contained in the first section of his Oeuvres and to R. Abel's "Uberblick über die geschichtliche Entwicklung der Lehre von der Infektion, Immunität und Prophylaxe" in Vol.1,1, of the Handbuch der Pathogenen Mikroorganismen (Kolle-Kraus-Uhlenhuth). A detailed exposition is to be found in E.O. von Lippmann's Urzeugung und Lebenskraft, 1933. This book only reports opinions, not experiments, on the subject of our question, but it gives an abundance of information on the literature, mostly in footnotes. For Aristotle's conceptions see pp.8-11 in it.

Page : The proposition Omne vivum ex ovo is generally falsely attributed to W. Harvey. The proposition reported in the text is quoted by R. Abel (op. cit., p.12). Von Lippmann interprets it differently with reference to the fact that for worms, insects and lower plants Harvey expressly admitted generatio aequivoca. (Lippmann, pp.58-59)

Page : Lippmann quotes from Bonnet's Considérations sur les corps organisés, Amsterdam, 1762, the sentence: "It is impossible for an animal to be formed in the same way as a crystal." (p.73)

Pages : Pasteur was clearly the first to demonstrate that the blood and urine of healthy humans and animals, taken and kept sterile, shows no formation of organisms; and similarly, grape juice shows no fermentation. Against Donné and others he proved that bacteria in hens' eggs with intact shells were not necessarily spontaneously generated, but might have entered the egg in the oviduct prior to shell formation. He was able to show that bacteria introduced into the hen's internal sex organs could later be found in the complete egg.

Pages : Ferd. Cohn declared his acceptance of the theory of the eternity of life and of panspermism. (According to Dastre, p.245, this expression originates from du Bois-Raymond.)

Page : Heat resistance of bacteria - see below.

Page : Monads: an older name for various simple organisms - Myxomycetes, Flagellata, Micrococcus, etc. (According to Guttmann, Medizinische Terminologie)

Page : The expression "archebiosis", for genesis of life, derives from Haeckel.

Page : According to Dastre (p.244) the cosmozoa hypothesis was first formulated by a French writer, le Comte de Salles-Guyon.

Pages : In the paper quoted Preyer coined the dictum Omne vivum e vivo! This too is often erroneously ascribed to Harvey.

Page : Ruelf formulated a similar hypothesis to Allen's in 1907 ("Über das organische Assimilations-produkt", in Zeitschrift für Allgemeine Physiologie, Vol,6). But according to V. Franz (Protobiologie und Organisations-stuffen, pp.1-2), from whom I took these accounts, Ruelf was thinking rather "of high electrical tensions in the atmosphere, whose effect was comparable with that in Loeb's formaldehyde synthesis from carbon dioxide and water with the aid of a static electrical discharge." Upon this formaldehyde- and E. Fischer's protein-syntheses, B. Weiss (Zentralblatt für Physiologie, 21, 1907, p.74) also bases a hypothesis of the "chemical process of spontaneous generation" which was then able to proceed to the "biological life-process" through the addition of chlorophyll, since the latter made possible "the transition from electrical to sunlight energy".

Page : Paul Jensen defends himself in Erkenntnis, Vol,4, 1934, p.214, against Lippmann (p.114) who drew a parallel between his theory and Haeckel's.

> "According to my theory the primordial living system arose at a time when, according to generally accepted ideas the surface of the earth was still molten. They were, therefore, something very different to the "anuclear lumps of protein" in Haeckel's theory of spontaneous generation. I regard it as idle to form more exact conceptions of the primordial living systems." (Jensen, p.214)

Page : In the article in the Umschau which I have mentioned ("Über die angebliche künstliche Erzeugung von Lebewesen"), as well as elsewhere; Wilhelm Roux has given a "functional" definition of the concept "living organism" which I shall quote here according to Kammerer (Allg. Biologie, pp.45-46), who gives Roux's paper "Die Selbst-regulation" (Nova Acta der Leop.-Carol. Akademie, Vol.C, No.2, Halle, 1914) as his source:

> "A definition of a living organism can, at present, only be made on the basis of the functions of organisms as they are now known to us. Accordingly, living organisms are, at the least, natural bodies which (1) absorb foreign bodies (auto-nutrition) and (2) transform these into their own substance, assimilation (auto-assimilation); (3) modify

themselves, by internal processes (dissimilation), and, at the same time, (4) by auto-secretion of that which has been modified; (5) through auto-substitution of this, by nutriment-absorption and auto-assimilation, maintain themselves entirely or almost entirely unchanged; (6) through over-compensation in the substitution of what has been used up, are able to grow (auto-growth); (7) from mainly internal causes are able both to move (automatic reflex movement) and (8) to divide (auto-division, auto-propagation); and in the process (9) to transfer their characteristics completely to the division products (heredity); finally (10), the capacity to return to the previous equilibrium after any one of these functions has been disturbed (auto-regulation). It remains to be emphasised that all these long-known functions belong together, and that, according to their particular nature, they are essentially conditioned, "determined", within the living organism itself, even if their "consummation" is in many ways dependent upon external factors and the functions can be modified somewhat according to their nature by external influences. Their totality causes that which is specific to the living organism ...

Only a formation which exhibits the sum of all these functions should be described as even one of the lowest forms of life. But this can then be done without any consideration of origin or of further special chemical and physical constitution."

Roux postulated "the genesis of the primeval life through successive development of the basic functions". The primal living organism, which, after Haeckel, he called "probionts", passed, during gigantic periods of time, through certain stages of development. Roux called the (hypothetical) formations which had developed the first four of the functions named, "isoplasson". On the addition of "auto-movement" and "reflex movement" they are called "auto-kineon". Then follow "auto-merizonts" (self-dividers), and then "idio-autoplasonts" (self-formers), with which the stage of the lowest forms of life known to us is reached. (Vide, Roux, Der Züchtende Kampf der Teile im Organismus, 1881, reprinted in Gesammelte Abhandlungen über Entwicklungsmechanik der Organismen, Vol.1, 1895, p.409 et seq. and Vol.2, 1895, pp.83-85; also No.1 of the Vorträge und Aufsätze über die Entwicklungsmechanik der Organismen:- "Die Entwicklungsmechanik, ein neuer Zweig der Biologischen Wissenschaft", 1905, p.105 et seq.)

Page : Rhumbler (1906) remarked, regarding Lehmann's investigations, that already in 1902 he had described, in his "mercury excrescenses", formations of another kind which were capable of dividing and showed abundant longitudinal growth

without increase in thickness. Mercury was dropped into a 5% chromic acid solution, forming a thin layer of mercuric chromate on the surface of the drops. The pressure of this membrane on the interior of the drop caused the mercury to burst out through the membrane, when it again reacted with the chromic acid. A much greater number of forms is produced in this way than with fluid crystals. (Vide, Rhumbler, 1906, p.18-22)

Page : Jaques Loeb looked upon "living organisms as chemical machines consisting essentially of colloidal substances", (Vorlesunben über die Dynamik der Lebenserscheinungen, 1906), but paid no particular attention to the question of spontaneous generation. In The Mechanistic Conception of Life (1912) he said:

> "Nothing at present, however, suggests that the artificial production of living matter lies beyond the possibilities of science ... We must either succeed in producing living matter artificially or we must find out why this is impossible." (pp.5-6)

The views of le Dantec which I have quoted only represent a possible idea. His final attitude (as defined in the book quoted) corresponds to that of Loeb.

Page : In an article entitled "Zelle, Bakterium und mikroskopisch unsichtbare Lebewesen" (Berliner Tierärztlicher Wochenschrift, 1936, pp.501-503) Glässer expressed the opinion that "protobionts" of about the size of molecules represented the beginning of life. Cells must be regarded as much more highly organised. The minute disease excitants found in viruses must have previously been free-living organisms.

Phenomena and problems of bacteriophagy were dealt with in particular detail by R. Otto and H. Munter in Handbuch der pathogenen Mikroorganismen, Vol.1, No.1, 1929, pp.353-436. In the opinion of the authors, the "functional" characteristic of assimilation and adaptation in bacteriophagi have not been definitely established experimentally. (They use Wilhelm Roux's definition of life as quoted above)

Heat resistance of bacteria: The Tabulae biologicae, Vol.2, 1925, published by C. Oppenheimer and L. Pincussen, contains (p.10 et seq.) tables of the "upper temperature limits of life" set out by Aristides Kanitz. Some extracts follow:

| Species. | Temp. | Behaviour. | Observers. |
|---|---|---|---|
| Bac. subtilis, spores. | 80° | Can survive 75 hrs. | O.Blau & A.Meyer. |
| | 140° | " " 30 sec. | |
| Bac. robur, " | 80° | " " 27 hrs. | " |
| | 130° | " " 14 sec. | " |
| Bac. anthracis, " | 90.4° | " " 14.7 min. | Ballner, Biggelow & Esty. |
| | 105.3° | " " 8.43 " | |
| Thermophilic bacteria. | 100° | " " 788 min. | " |
| | 140° | " " 0.9 min. | " |
| Rotifer tardigradus. | 44° | Dries up. | J.S.Rahm |
| | 150° | Can survive for a short time if dry, but fatal to animals which have been moistened. | " |

The same facts and figures were given by Hans Przibram in Temperatur und Temperatoren im Tierreiche, 1923. Where not otherwise stated, heat is dry.

But E. Gotschlich wrote (op. cit., p.197):

"On the other hand, spores exist in hay dust, cow dung, etc., which may be boiled for many hours without reduction of their capacity for germination and, in particular, prsent difficulties in the sterilisation of milk. (Vide, C. Fluegge, Ztschr. f. Hygiene, Vol.17) The greatest resistance known is that reported by C. Zettnow (Centralblatt für Bakteriologie, Vol.66, 1912, p.131) of spores isolated from beet sugar sugar waste. They were still alive after 30 minutes dry heat at 300°C, 25 hours in a jet of steam, and 30 minutes boiling in 10% sodium hydroxide solution. Objects soiled with earth, refuse, etc., and in which the presence of such highly resistant spores has to be reckoned with (potatoes, milk) can only be sterilised

with certainty in the autoclave. It is of great theoretic-:al interest that such a high resistance was only observed by C. Zettnow in spores cultured on neutral agar, but not when they were cultured on the ordinary, slightly alkaline agar. This difference is attributed to the varying degree of swelling of their protoplasm."

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